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I. CONSCIENCE IN ANIMALS.

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AMONG several other topics which are dealt with in an interesting article headed "Animal Depravity" that appeared in the number of this Journal for October last, the writer alludes to the question as to whether or not the rudiments of a moral sense are discernible in animals. This question I consider to be of so much importance from a psychological point of view that, although a great deal of observation which I have directed towards its enlightenment has hitherto yielded but small results, I am tempted to publish the latter, such as they are, in the hope that, if they serve no better end, they may perhaps induce some other observers to bestow their attention upon this very interesting subject.

I may first briefly state what I conceive to be the theoretical standing of the subject. At the present day, when the general theory of evolution is accepted by all save the ignorant or the prejudiced, the antecedent probability is overwhelming that our moral sense, like all our other psychological faculties, has been evolved. The question as to the *causes* of its evolution has been discussed in the "Descent of Man," and this with all the breadth of thought and force of fact so characteristic of the writings which have exerted an influence upon human thought more profound than has been exerted by the writings of any other single man—not even excepting Aristotle in Philosophy or Newton in Science. Mr. Herbert Spencer, also, has treated of this subject, and, if his wonderful "programme" is ever destined to attain completion, we may expect copious results when his great powers are brought to bear upon the "Principles of Morality." Meanwhile, however, we have ample evidence to render it highly probable that at any rate the leading causes in the development of our moral sense have had their origin in the social instincts. Indeed, to any one who impartially considers this evidence in the light of the general theory of evolution, it must appear well-nigh incredible that so considerable a body of proof can ever admit of being overcome. Nor is this all. Not only is it true that so much success has attended Mr. Darwin's method of determining synthetically the causes which have been instrumental in evolving the moral sense,* but, long before any scientific theory of evolution had been given to the world, our great logician—following in the track of Hume (whose part in this matter has not, I think, been sufficiently appreciated), Bentham, and others—proved analytically, to the satisfaction of all competent and impartial thinkers, that the moral sense is rooted in "the greatest amount of happiness principle" as its sustaining source. In other words, John Stuart Mill, by examining Conscience as he found it to exist in Man, showed that it depends upon the very principle upon which it ought to depend, supposing Mr. Darwin's theory—elaborated, be it remembered, without any reference to Mr. Mill's analysis, and arrived at by a totally different line of enquiry—concerning the causes of its evolution to be the true one.

Stronger evidence, then, as to the physical causes whose operation has brought human conscience into being, we could scarcely expect, in the present condition of physical science, to possess. It is unnecessary, however, in this place to enter into the details of this evidence, as almost every educated person must be more or less acquainted with them. I shall therefore pass on to the next point which concerns us—namely, supposing the causes of our moral sense to have had their origin in the social instincts, where and to what extent should we expect to find indications of an incipient moral sense in animals? First, then, what do we mean by conscience? We mean that faculty of our minds which renders possible remorse or satisfaction for past conduct, which has been respectively injurious or beneficial to others.† This, at least, is what I conceive conscience to be in its last resort. No doubt, as we find it in actual operation, the faculty in question has reference to ideas of a higher abstraction than that of the fellow man whom we have injured or benefitted. In most cases the moral sense has reference to the volitions of a Deity, and in others to the human race considered as a whole. But if the moral sense has been developed in the way here supposed, its root-principle must be that which has reference to ideas of no higher abstraction than those of parent, neighbour, or tribe. Now, even in this its most rudimentary phase of development, conscience pre-supposes a comparatively high order of intelligence as the prime condition of its possibility. For not only does the faculty as above defined require a good *memory* as a condition essential to its existence, but—what is of much greater importance—it also requires the power of *reflecting upon past conduct*; and this, it is needless to say, appears to be a much rarer quality in the psychology of animals than is mere memory.

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* I willingly endorse the just tribute recently paid to this part of Mr. Darwin's work by Prof. Clifford:—"To my mind the simplest and clearest and most profound philosophy that was ever written upon this subject is to be found in chapters ii. and iii. of Mr. Darwin's 'Descent of Man.'"—*Fort. Rev.*, p. 794.

† For reasons which may easily be gathered from the next succeeding sentences, I omit *conscientious* ideas of what is due to *self*.

Thus, if Mr. Darwin's theory concerning the origin and development of the moral sense is true, we should not expect to find any indications of this faculty in any animals that are too low in the psychological scale to be capable of reflecting upon their past conduct. Whether this limitation does not exclude all animals whatever is a question with which I am not here concerned. I merely assert that if the theory in question is the true one, and if no animals are capable of reflecting upon their past conduct, then no animals can possess a moral sense, properly so-called. And from this of course it follows that if any animals can be shown to possess a moral sense, they are thereby also shown to be capable of reflecting upon their past conduct.

Again, if Mr. Darwin's theory concerning the origin and development of the moral sense is true, it is self-evident that we should not expect to find any indications of this faculty in animals that are either unsocial or unsympathetic. Supposing the theory true, therefore, our search for animals in which we may expect to find any indications of a moral sense is thus seen to be very restricted in its range: we can only expect to find such indications in animals that are highly intelligent, social, and sympathetic. Since by the hypothesis conscience requires a comparatively rare collocation of conditions for its development, we must expect to find it a comparatively rare product.

Lastly, as it is quite certain that no animal is capable of reflecting upon past conduct in any high degree, and as we have just seen that the moral sense depends upon the faculty of so reflecting, it follows that we cannot expect to find any animal in which the moral sense attains any high degree of development.

We are now in a position to draw some important distinctions. There are several instincts and feelings which, when expressed in outward action, more or less simulate conscience (so to speak), but which it would be erroneous to call by that name. For instance, the maternal instinct, although it leads in many cases to severe and sustained self-denial for the benefit of the offspring, is nevertheless clearly distinct from conscience. The mother in tending her young does so in obedience to an inherited instinct, and not from any fear of subsequent self-reproach if she leaves her family to perish. She follows the maternal instinct, so long as it continues in operation, just as she would follow any other instinct; and it is, as it were, a mere accident of the case that in this particular instance the course of action which the instinct prompts is a course of action which is conducive to the welfare of others. An illustration will render this distinction more clear. In his chapter on the "Moral Sense" Mr. Darwin alludes to the conflict of instincts which sometimes occurs in swallows when the migratory season overtakes a late brood of young birds; at such times "swallows, house martins, and swifts frequently desert their tender young, leaving them to perish miserably in their nests." And further on he remarks—"When arrived at the end of their long journey, and the migratory instinct has ceased to act, what an agony of remorse the bird would feel, if, from being endowed with great mental activity, she could not prevent the image constantly passing through her mind, of her young ones perishing in the bleak north from cold and hunger." In other words, if we could suppose the mother bird under such circumstances to be capable of *reflecting upon her past conduct*, and as a consequence suffering an "agony of remorse," then the bird might properly be said to be *conscience-stricken*. And if we could suppose the bird, while still brooding over her young ones, to foresee the agony of remorse she would subsequently feel if she now yields to the stronger instinct by deserting her young, then the bird might properly be said to be acting *conscientiously*.

Again, mere fear of punishment must not be confused with conscience—it being of the essence of conscientious action that it should be prompted by feelings wholly distinct from fear of retaliation by the object of injury, whether by way of punishment or revenge. Conscience must be capable of effecting its own punishment if violated; otherwise the principle of action, whatever it may be, must be called by some other name.*

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* Of course I recognise fear of punishment as an important factor in the *original constitution* of the moral sentiment; but, for reasons stated at the end of this article, we must, when treating of animal psychology, eliminate this factor when *conscience has become sufficiently developed to be "a law to itself."*

It is evident that conscience, as we find it in ourselves, is distinct from love of approbation and fear of disapprobation. Nevertheless, if our hypothesis concerning the development of the moral sense is the true one, we should expect that during the early phases of that development love of approbation and fear of disapprobation should have played a large part in the formation of conscience. For although, by the hypothesis, it is sympathy and not self-love that constitutes the seat of the moral sense, still the particular manifestations of self-love with which we are now concerned—viz., desire of approbation and dislike of the reverse—would clearly be impossible but for the presence of sympathy. "Mr. Bain has clearly shown that the love of praise, and the strong feeling of glory, and the still stronger horror of scorn and infamy, 'are due to the workings of sympathy.'"* I think, therefore, that in testing—by observations upon the lower animals—the truth of Mr. Darwin's theory concerning the genesis of conscience, it would be no valid objection to any satisfactory instances of conscientious action in an animal to say that such action is partly due to a desire of praise or a fear of blame. This would be no valid objection, because, in the first place, it would in most cases be impossible to say how far the implication is true—how far the animal may have acted from pure sympathy or regard for the feelings of others, and how far from an admixture of sympathy with self-love; and in the next place, even if the implication be conceded wholly true, it would not tend to disprove the theory in question. If an animal's sympathies are so powerful that, even after being reflected through self-love, they still retain force enough to prompt a course of action which is in direct opposition to the more immediate dictates of self-love, then the sympathies of such an animal are hereby proved to be sufficiently exalted to constitute the beginnings of a conscience, supposing the theory which we are testing to be the true one.

Similarly, there is an obvious distinction in ourselves between injured conscience and injured pride. But if conscience has been developed in the way here supposed, it follows that in the rudimentary stages of such development the distinction in question cannot be so well defined. Pride presupposes consideration for the opinion of others, and this in turn—as we have just seen—presupposes sympathy, which is the foundation-stone of conscience. Now it is certain that long before we reach, in the ascending scale of animal psychology, intellectual faculties sufficiently exalted to admit even of our suspecting the presence of an incipient moral sense, we can perceive abundant indications of the presence of pride. And forasmuch as animals that are high in the psychological scale frequently exhibit a very profound appreciation of their own dignity, we may pretty safely conclude that in no case can we expect to find indications of a moral sense in an animal without a greater or less admixture of pride.

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* Descent of Man, p. 109 (1874). Mental and Moral Science, p. 254 (1868).

I will now sum up this rather tedious preamble:—From Mr. Darwin's theory concerning the development of conscience, it appears to follow that the presence of this faculty in animals must be restricted—if it occurs at all—to those which are intelligent enough to be capable in some degree of reflecting upon past conduct, and which likewise possess social and sympathetic instincts. From the first of these conditions it follows, supposing Mr. Darwin's theory true, that in the case of no animal should we expect to find the moral sense developed in any other than a low degree.

There is no reason to suppose any mere *instinct* (such as the maternal) due to conscience; for an instinct acquired by inheritance is obeyed blindly, in order to avoid the uncomfortable sensation which ensues in a direct manner if it is not so obeyed,—whereas conscience enforces obedience only through a process of reflection;* the uncomfortable sensation which non-obedience entails in this case being only brought about in an indirect manner through the agency of re-presentative thought.

Although conscience in man is independent of, or distinct from, love of approbation, fear of reproach, and sense of pride, there is no reason why we should suppose conscience in its rudimentary forms to be independent of these passions. On the contrary, I think we should expect a rudimentary form of conscience to be more or less amalgamated with such passions; for long before the faculty in question has attained the highly differentiated state in which we find it to be present in ourselves, it must (by the hypothesis) have passed through innumerable states of lesser differentiation in which its existence was presumably more and more bound up with that of those more primary social instincts from which it first derived its origin. To us conscience means a massive consolidation of innumerable experiences, inherited and acquired, of remorse following one class of actions and gratification their opposites; and this massive body of experience has reference to ideas of an abstraction so high as to extend far beyond the individual, or even the community, which our actions primarily affect. No wonder, therefore, that when any course of action is being contemplated, conscience asserts her voice within us as a voice of supreme authority, commanding us to look beyond all immediate issues, inclinations, and even sympathies, to those great *principles* of action which the united experience of mankind has proved to be best for the individual to follow in all his attempts to promote the happiness or to alleviate the misery of his race. But with animals, of course, the case is different. They start with a very small allowance of hereditary experience in the respects we are considering; they have very few opportunities of adding to those experiences themselves; they probably have no powers of forming abstract ideas; and so their moral sense, rudimentary in its nature, can never be exercised with reference to anything other than concrete objects—relation, companion, or herd.

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* *I. e., originally:* when once the *habit* of yielding obedience to conscience has been acquired, it becomes itself of the nature of an instinct—neglect to practise this habit giving rise immediately, or without any process of reflection, to an uncomfortable state of the mind.

We may now proceed to answer the question already propounded, namely—Supposing Mr. Darwin's theory concerning the origin of the moral sense to be true, where among animals should we expect to find indications of such a sense? I think reflection will show that the three essential conditions to the presence of a moral sense are only complied with among animals in the case of three groups—namely, dogs, elephants, and monkeys. I need not say anything about the *intelligence* or the *sociability* of these animals, for it is proverbial that there are no animals so intelligent or more social. It is necessary, however, to say a few words about sympathy.

In the case of dogs sympathy exists in an extraordinary degree. I have myself seen the life of a terrier saved by another dog which stayed in the same house with him, and with which he had always lived in a state of bitter enmity. Yet when the terrier was one day attacked by a large dog, which shook him by the back and would certainly have killed him, his habitual enemy rushed to the rescue, and after saving the terrier had great difficulty in getting away himself.

With regard to elephants I may quote the well-known instance from the "Descent of Man":—"Dr. Hooker informs me that an elephant, which he was riding in India, became so deeply bogged that he remained stuck fast until next day, when he was extracted by means of ropes. Under such circumstances elephants seize with their trunks any object, dead or alive, to place under their knees, to prevent their sinking deeper in the mud; and the driver was dread-fully afraid lest the animal should have seized Dr. Hooker and crushed him to death. But the driver himself, as Dr. Hooker was assured, ran no risk. This forbearance, under an emergency so dreadful for a heavy animal, is a wonderful proof of noble fidelity."*

Many cases of sympathy in monkeys might be given, but I shall confine myself to stating one which I myself witnessed at the Zoological Gardens.† A year or two ago there was an Arabian baboon and an Anubis baboon confined in one cage, adjoining that which contained a dog-headed baboon. The Anubis baboon passed its hand through the wires of the partition, in order to purloin a nut which the large dog-headed baboon had left within reach,—expressly, I believe, that it might act as a bait. The Anubis baboon very well knew the danger he ran, for he waited until his bulky neighbour had turned his back upon the nut with the appearance of having forgotten all about it. The dog-headed baboon, however, was all the time slyly looking round with the corner of his eye, and no sooner was the arm of his victim well within his cage than he sprang with astonishing rapidity and caught the retreating hand in his mouth. The cries of the Anubis baboon quickly brought the keeper to the rescue, when, by dint of a good deal of physical persuasion, the dog-headed baboon was induced to leave go his hold. The Anubis baboon then retired to the middle of his cage, moaning piteously, and holding the injured hand against his chest while he rubbed it with the other one. The Arabian baboon now approached him from the top part of the cage, and, while making a soothing sound very expressive of sympathy, folded the sufferer in its arms—exactly as a mother would her child under similar circumstances. It must be stated, also, that this expression of sympathy had a decidedly quieting effect upon the sufferer, his moans becoming less piteous so soon as he was enfolded in the arms of his comforter; and the manner in which he laid his cheek upon the bosom of his friend was as expressive as anything could be of sympathy appreciated. This really affecting spectacle lasted a considerable time, and while watching it I felt that, even had it stood alone, it would in itself have been sufficient to prove the essential identity of some of the noblest among human emotions with those of the lower animals.

If there is any validity in the foregoing antecedent reflections, all who have the opportunity should make a point of observing whether any indications of conscience are perceptible in monkeys, elephants, or intelligent dogs. My own opportunities of observation have been restricted to the last of these animals alone, so I shall conclude this article by giving some instances which appear to me very satisfactorily to prove that intelligent and sympathetic dogs possess the rudiments of a moral sense.

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* See also HOOKER's Himalayan Journal, vol. ii., p. 333 (1854).

† I hope it is unnecessary to say that in detailing this and all the subsequent incidents, I carefully avoid exaggeration or embellishment of any kind.

I have a setter just now which has been made a pet of since a puppy. As he has a very fine nose, and is at liberty to go wherever he pleases, he often finds bits of food which he very well knows he has no right to take. If the food he finds happens to be of a dainty description, his conscientious scruples are overcome by the temptations of appetite; but if the food should be of a less palatable kind, he generally carries it to me in order to obtain my permission to eat it. Now, as no one ever beats or even scolds this dog for stealing, his only object in thus asking permission to eat what he finds must be that of quieting his conscience. It should be added that when he brings stolen property to me it does not always follow that he is allowed to keep it.

This same animal, when I am out shooting with him, sometimes of course flushes birds. When he does so he immediately comes to me in a straight line, carrying his head and tail very low, as if to ask for pardon. Although I speak reproachfully to him on such occasions, I scarcely ever chastise him; so it cannot be fear that prompts this demeanour.

One other curious fact may here be mentioned about this dog. Although naturally a very vivacious animal, and, when out for a walk with myself or any other young person, perpetually ranging about in search of game, yet if taken out for a walk by an elderly person he keeps close to heel all the time—pacing along with a slow step and sedate manner, as different as possible from that which is natural to him. This curious behaviour is quite spontaneous on his part, and appears to arise from his sense of the respect that is due to age.

The writer of the article on "Animal Depravity" makes the following quotation from an article of mine in "Nature" (vol. xii., p. 66):—"The terrier used to be very fond of catching flies upon the window-panes, and if ridiculed when unsuccessful was evidently much annoyed. On one occasion, in order to see what he would do, I purposely laughed immoderately every time he failed. It so happened that he did so several times in succession.—partly, I believe, in consequence of my laughing,—and eventually he became so distressed that he positively *pretended* to catch the fly, going through all the appropriate actions with his lips and tongue, and afterwards rubbing the ground with his neck as if to kill the victim : he then looked up at me with a triumphant air of success. So well was the whole process simulated that I should have been quite deceived, had I not seen that the fly was still upon the window. Accordingly I drew his attention to this fact, as well as to the absence of anything upon the floor ; and when he saw that his hypocrisy had been detected he slunk away under some furniture, evidently very much ashamed of himself."

Upon this case the author of the article on "Animal Depravity" very properly observes :—"This last point is most significant, fully overturning the vulgar notion of the absence of moral life in brutes, and of their total want of conscience." I think this observation is warranted by the facts, for although I have heard it objected that the feeling displayed by the terrier in this case was that of wounded pride rather than of wounded conscience, still, from what has been previously said concerning this distinction in the case of animals, it will be seen that in this instance it is not easy to draw the line between these two sentiments.

The following instances, however,—all of which occurred with the terrier just mentioned—are free from this difficulty.

For a long time this terrier was the only canine pet I had. One day, however, I brought home a large dog, and chained him up outside. The jealousy of the terrier towards the new-comer was extreme. Indeed I never before knew that jealousy in an animal could arrive at such a pitch ; but as it would occupy too much space to enter into details, it will be enough to say that I really think nothing that could have befallen this terrier would have pleased him so much as would any happy accident by which he might get well rid of his rival. Well, a few nights after the new dog had arrived, the terrier was, as usual, sleeping in my bed-room. About 1 o'clock in the morning he began to bark and scream very loudly, and upon my waking up and telling him to be quiet he ran between the bed and the window in a most excited manner, jumping on and off the toilette-table after each journey, as much as to say—"Get up quickly; you have no idea of what shocking things are going on outside." Accordingly I got up, and was surprised to see the large dog careering down the road : he had broken loose, and, being wild with fear at finding himself alone in a strange place, was running he knew not whither. Of course I went out as soon as possible, and after about half-an-hour's work succeeded in capturing the runaway. I then brought him into the house and chained him up in the hall; after which I fed and caressed him with the view of restoring his peace of mind. During all this time the terrier had remained in my bed-room, and, although he heard the feeding and caressing process going on downstairs, this was the only time I ever knew him fail to attack the large dog when it was taken into the house. Upon my re-entering the bed-room, and before I said anything, the terrier met me with certain indescribable grinnings and prancings, which he always used to perform when conscious of having been a particularly good dog. Now I consider the whole of this episode a very remarkable instance in an animal of action prompted by a sense of *duty*. No other motive than the voice of conscience can here be assigned for what the terrier did: even his strong jealousy of the large dog gave way before the yet stronger dread he had of the remorse he knew he should have to suffer, if next day he saw me distressed at a loss which it had been in his power to prevent. What makes the case more striking is, that this was the only occasion during the many years he slept in my bed-room that the terrier disturbed me in the night-time. Indeed the scrupulous care with which he avoided making the least noise while I was asleep, or pretending to be asleep, was quite touching,—even the sight of a cat outside, which at any other time rendered him frantic, only causing him to tremble violently with suppressed emotion when he had reason to suppose that I was not awake. If I overslept myself, however, he used to jump upon the bed and push my shoulder gently with his paw.

The following instance is likewise very instructive. I must premise that the terrier in question far surpassed any animal or human being I ever knew in the keen sensitiveness of his feelings, and that he was never beaten in his life.* Well, one day he was shut up in a room by himself, while everybody in the house where he was went out. Seeing his friends from the window as they departed, the terrier appears to have been overcome by a paroxysm of rage; for when I returned I found that he had torn all the bottoms of the window-curtains to shreds. When I first opened the door he jumped about as dogs in general do under similar circumstances, having apparently forgotten, in his joy at seeing me, the damage he had done. But when, without speaking, I picked up one of the torn shreds of the curtains, the terrier gave a howl, and rushing out of the room, ran up stairs screaming as loudly as he was able. The only interpretation I can assign to this conduct is, that his former fit of passion having subsided, the dog was sorry at having done what he knew would annoy me; and not being able to endure in my presence the remorse of his smitten conscience, he ran to the farthest corner of the house crying *peccavi* in the language of his nature.

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* A reproachful word or look from me, when it seemed to him that occasion required it, was enough to make this dog miserable for a whole day. I do not know what would have happened had I ventured to strike him; but once when I was away from home a friend used to take him out every day for a walk in the park. He always enjoyed his walks very much, and was now wholly dependent upon this gentleman for obtaining them. (He was once stolen in London through the complicity of my servants, and never after that would he go out by himself, or with anyone whom he knew to be a servant.) Nevertheless, one day while he was amusing himself with another dog in the park, my friend, in order to persuade him to follow, struck him with a glove. The terrier looked up at his face with an astonished and indignant gaze, deliberately

I could give several other cases of conscientious action on the part of this terrier, but as the present article is already too long I shall confine myself to giving but one other case. This, however, is the most unequivocal instance I have ever known of conscience being manifested by an animal.

I had had this dog for several years, and had never—even in his puppyhood—known him to steal. On the contrary, he used to make an excellent guard to protect property from other animals, servants, &c., even though these were his best friends.* Nevertheless, on one occasion he was very hungry, and in the room where I was reading and he was sitting, there was, within easy reach, a savoury mutton chop. I was greatly surprised to see him stealthily remove this chop and take it under a sofa. However, I pretended not

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turned round, and trotted home. Next day he went out with my friend as before, but after he had gone a short distance he looked up at his face significantly, and again trotted home with a dignified air. After this my friend could never induce the terrier to go out with him again. It is remarkable, also, that this animal's sensitiveness was not only of a selfish kind, but extended itself in sympathy for others. Whenever he saw a man striking a dog, whether in the house or outside, near at hand or at a distance, he used to rush to the protection of his fellow, snarling and snapping in a most threatening way. Again, when driving with me in a dog-cart, he always used to seize the sleeve of my coat every time I touched the horse with the whip.

* I have seen this dog escort a donkey which had baskets on its back filled with apples. Although the dog did not know that he was being observed by anybody, he did his duty with the utmost faithfulness; for every time the donkey turned back its head to take an apple out of the baskets, the dog snapped at its nose; and such was his watchfulness, that, although his companion was keenly desirous of tasting some of the fruit, he never allowed him to get a single apple during the half-hour they were left together. I have also seen this terrier protecting meat from other terriers (his sons), which lived in the same house with him, and with which he was on the very best of terms. More curious still, I have seen him seize my wristbands while they were being worn by a friend to whom I had temporarily lent them.

to observe what had occurred, and waited to see what would happen next. For fully a quarter of an hour this terrier remained under the sofa without making a sound, but doubtless enduring an agony of contending feelings. Eventually, however, conscience came off victorious, for, emerging from his place of concealment and carrying in his mouth the stolen chop, he came across the room and laid the tempting morsel at my feet. The moment he dropped the stolen property he bolted again under the sofa, and from this retreat no coaxing could charm him for several hours afterwards. Moreover, when during that time he was spoken to or patted, he always turned away his head in a ludicrously conscience-stricken manner. Altogether I do not think it would be possible to imagine a more satisfactory exhibition of conscience by an animal than this; for it must be remembered, as already stated, that the particular animal in question was never beaten in its life.*

II. NATURE'S SCAVENGERS.

IN these days of Sanitary Reform it may be interesting to examine what is the real state and value of existing natural arrangements for the removal of nuisances, and for the disinfection of the waters and the atmosphere. The subject may, fortunately, now be discussed without calling forth those strong expressions of affected disgust with which it would have been greeted not many years ago. It has a very obvious practical bearing: we have to consider what is the actual utility of Nature's Scavengers,—in how far we may trust to their action,—when and where we should assist and cherish them, and under what circumstances we should seek to supersede them altogether. Nor is the question without a speculative interest. The efficiency and completeness, or the opposite qualities of Nature's agencies for dealing with refuse, may throw some valuable cross-lights upon the origin of species, and indeed upon the whole debate now pending between the old and the new schools of Natural History.

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* This latter point is most important because, although the moral sentiment in its incipient stages undoubtedly depends in a large measure upon fear of punishment, still in its more developed state this sentiment is as undoubtedly independent of such fear (Cf. BAIN, "Mental and Moral Science," pp. 456-9, 1875); and forasmuch as in our analysis of animal psychology we can be guided only by the study of outward actions, and forasmuch as the course of action prompted by direct fear of punishment will nearly always be identical with that prompted by true conscience, it is of the first importance to obtain cases such as the above, in which mere dread of punishment cannot even be suspected to have been the motive principle of action.

We will at once begin by examining what living agencies exist for the removal of carrion, excrement, and other putrid or putrescent matters on land. Among the Mammalia we find no inconsiderable number of species which feed upon decomposing animal matter. The Felidæ certainly prefer living prey, but in case of need even the lion of Algeria will compromise the dignity with which he has been invested by closet-naturalists by devouring putrid carcases, and the very *immondices* which generally accumulate outside the walls of a town. The true panther (*Felis pardus*), also, in case of need, preys upon carrion, and will even dig up and devour dead bodies. The Bengal tiger is said to eat the dung of the rhinoceros. But the genuine carrion-eaters are the Canidæ —the jackal, the wolf, and the domestic dog. These animals appear to like their food tainted; they will roll themselves upon a putrid carcase, and even when well fed they will greedily devour human excrement. No species or variety is more given to this loathsome diet than the King Charles spaniel. But when such substances are eaten by any animal we have to ask whether the nuisance is really overcome, or merely altered in its form? The latter view is much more consonant with truth. The secretions and excretions of a carrion-feeder are in quality little better than the refuse eaten. A certain quantity is, indeed, consumed in the body of the filth-devourer, and makes its reappearance in the shape of inorganic compounds, such as carbonic acid, watery vapour, and ammonia. But all that is given off in complex organic combinations is noisome in the extreme, and rapidly passes into a state of energetic putrefaction. The disgusting odour of the wolf, the hyæna, the vulture, and even of the domestic dog when stretched out before a cheerful fire, cannot escape recognition. These creatures, therefore, occupy an intermediate rank. As scavengers they are often useful, but not perfect. They do not propagate and increase nuisance, but neither do they entirely suppress it when thrown in their way.

There is, however, one instance, at least, of perfect scavenging among beasts of prey. We refer to the habit of the domestic cat in scraping earth over her excrements. This custom does not extend to the whole even of the Felidæ, but it re-appears in a rudimentary—or perhaps obsolescent—form in the dog, who generally gives two or three random scrapes or kicks with his hind legs on such occasions —a ceremony which, as now performed, can be of no use either to the animal himself or to any other creature.

In how far the omnivorous rodents—such as rats and mice—may be regarded as scavengers is somewhat doubtful. The best claim may be made for the grey or Norway rat (Hanoverian rat of Waterton), who is very fond of taking up his abode on the premises of knackers, bone-boilers, &c., and sometimes succeeds in penetrating into family vaults for the purpose of gnawing human remains. The black rat and the common mouse may occasionally devour putrid animal matter under the pressure of scarcity, but it by no means forms their ordinary or favourite diet. Still, animals of this tribe are, at the best, very unacceptable scavengers, from considerations which will be fully detailed when we come to speak of carrion-flies.

Among birds we find two distinct main groups of carrion-eaters—the vultures and the crows. Both execute their task in exactly the same manner as the Canidæ. They exhale from their skins the same detestable odour, and their dung is offensive in the extreme; but as they are not given to injure man—either in his person or to any appreciable extent in his property—they are, as scavengers, far superior to wolves, jackals, hyænas, panthers, and rats, and they may justly claim immunity from persecution, or even positive protection. Another bird, though very promiscuous in its diet, has some title to be regarded as a scavenger: we refer to the domestic duck, which will feast greedily upon putrid matter, the remains of its own kindred not excluded. Even tumours, the dressings from ulcers, and other the foulest *rejectamenta* of hospitals, are not disdained by these birds, which yet, in England, serve for the very type of all that is delicate.

Among reptiles we are unable to point out any species of scavenging habits. Although the great majority of lizards and serpents are purely carnivorous, they seek invariably living prey. This the writer was able to establish most conclusively, as far at least as European species are concerned, by a long series of observations made upon a numerous colony of these creatures, confined in a pit somewhat resembling a melon-frame, but filled with peaty earth, stones, and bushes of heath and of *Ledum palustre*. Small dead animals, such as mice, shrews, small birds, &c., if thrown into the pit, were completely disregarded by the snakes; but if a live mouse was thrown in, all the venomous inmates were up in arms in a moment, until one of their number had given the fatal bite. On one occasion only a large male viper, who had been very restless for some days, was observed carrying a limb of a dead bird about in his mouth;

he made, however, no attempt to swallow it. The alligators of the western hemisphere are said to bury their prey in the mud of the beds of rivers for some time before they devour it—a notion which is strenuously combatted by Waterton; but this habit, even if verified, scarcely entitles them to be regarded as regular eaters of carrion.

The amphibians do not appear to consume any putrescent or putrid matter, whether of animal or vegetable origin.

The more widely spread are scavenging habits among fishes, a very large proportion of which partake almost indifferently of organic matter, whether living or lifeless, fresh or decomposing.

It is, however, not amongst vertebrate animals, but among the Articulata, and especially among insects, that we find the most active and powerful scavengers. Here immense numbers and great voracity more than compensate for smallness of size.

The most numerous and the most efficient of insect scavengers are to be found in the so-called "order" Coleoptera.* Amongst these insects it is easier to particularise those which do not, wholly or in part, subsist upon vegetable and animal refuse than those which do. We shall, therefore, not furnish a catalogue of scavenging families and genera, but merely point out the most important. Perhaps the highest rank may be claimed by the Silphidæ. These insects not merely feed upon carrion, both when larvæ and in the adult state, but one of their genera—the *Necrophori*, or sexton-beetles—bury putrescent animal matter as food for their young. Wherever they find a small dead animal—mouse, frog, bird, &c.—or a fragment of some larger carcase which has been neglected by hyænas, jackals, vultures, and the like, they deposit their eggs therein, and then dig away the earth from below it, and cover it up. Their manner of proceeding has been so well described in various works on Natural History† that it need not be repeated here. But the quantity of matter which they thus inter deserves particular attention. A single sexton-beetle has been known to bury a mole forty times its own weight. Four beetles have been seen burying a crow, which would certainly exceed their united weights in a still higher ratio. Thus not only is nuisance prevented, but the earth is enriched with an excess of matter more than can be consumed by the larvæ. Nor is the mischief of putrefaction merely deferred or altered in form, as in the case of many carrion-feeders, since the excrements of the larvæ are absorbed by the earth, as well as the gases and vapours resulting from the decomposition of the dead body.

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* It must be ultimately admitted that the divisions Coleoptera, Lepidoptera, &c., are of far higher rank than orders. The coleopterous group Adephaga (Clairville) seems to be an order, equal in rank to Carnivora amongst mammals.

† For instance, RENNIE'S Insect Architecture, p. 233, and KIRBY and SPENCE, ii., 350. See also Act. Acad. Berolin, 1752.

Before we pronounce any scavenger perfect we must be sure that he confines himself solely to his useful, though repulsive task, and does not go about bedaubed with filth, disseminating the seeds of putrefaction, and probably of disease. Tried by this test the sexton-beetle is justified. He does not intrude into our dwellings, settle on our food, and buzz about our persons, contaminating whatsoever he touches. He removes, without propagating, nuisance, and the removal is not attended with any drawback or set-off. We must therefore declare him an admirable, unimpeachable scavenger, wishing him every success in his operations, and full immunity from the competition of quacks and bunglers. But with all these good qualities—perhaps we must even say because of them—he barely holds his own in the struggle for existence, and seems to us to be decidedly decreasing in numbers. An animal which feeds on one kind of food only is naturally at a disadvantage if it has to compete for the means of existence against omnivorous species. The *Necrophorus*, feeding only on carrion, is "underbidden" by creatures which can feed upon almost any kind of organic matter, and which are perfectly ready to deposit their ova in fresh flesh if they can find nothing already tainted. This is, we submit, a crucial case, deciding the comparative merits of the new and of the old natural history, and as such we shall have to refer to it again. No less does it exclude certain applications of the doctrine of "Natural Selection" made by political economists. We have still to notice the extreme limitation of the burying propensity. The other genera of the Silphidæ, though preying upon carrion* and depositing their eggs in the same material, do not bury, and are consequently of much less value as removers of nuisances. Now, if the burying propensity be an instinct especially implanted in these insects in order to preserve the air from taint, why is it restricted to one small group—neither rich in species nor in individuals—among an extensive family of carrion-feeders? Might we not rather expect that all the Silphidæ would bury, whenever, at least, they met with a carcase of suitable magnitude and in a fitting locality? If, therefore, we accept the point of view of the old natural history, we are forced to admit that an instinct which would have been beneficial both to these insects and to the world at large has been inexplicably withheld from them. Beneficial, we say, to the insects themselves, because larvæ in a piece of buried carrion are—all things considered—safer and more likely to reach maturity than if the carrion had been allowed to remain on the surface of the earth. In this latter case the larvæ are exceedingly liable to be picked out and consumed by birds, or the whole piece of carrion may at any moment be devoured by some passing dog, wolf, or hyæna. Beneficial, also, to the world at large—for if the burial of offensive matter secures the air from taint, it is surely important that all such matter should be buried, and not merely a small part.

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* Some of the species of *Silpha* attack living prey. MacLeay states that *Silpha 4-punctata* ascends oaks in pursuit of caterpillars. We have captured it on oaks in Dunham Park, near Manchester, under circumstances which decidedly favour this view.

We cannot especially examine the families of Sphæridiidæ, Histeridæ, and the vast group of Brachelytra, rove-beetles or "devil's coach-horses," containing nearly 800 British species. All these derive a part, at least, of their support from decaying animal and vegetable matter, and may be considered as good scavengers of the second rank—*i.e.*, devourers of filth which do not disseminate pollution. But we must turn to a more remarkable and interesting tribe of scavengers, the Saprophagous Lamellicornes of MacLeay, of which the common dung-beetle may be taken as the type.

If the reader, when taking a stroll in the fields during the spring or early summer, turns over with his stick a deposit of horse- or cow-dung, he will—except accustomed to entomological explorations—be astonished at the number and variety of living beings presented to his view. If the dung lies upon soft ground, and is neither too recent nor too old and dried up, he may often find beetles of a dozen or more species, all making arrangements for keeping up the circulation of matter. Whilst the dung itself is tenanted by *Staphylini*, *Histers*, *Sphæridiæ*, and their maggots, in the ground beneath he will generally see several round holes, varying from a few lines to nearly an inch in diameter, and extending a considerable depth into the ground.*

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* The burrows of *Typhæus vulgaris*, the three-horned dung-beetle, are bored exceedingly neatly. I cannot help suspecting that, after ground has once been broken, the three thoracic horns of this species—which all point forwards, and are nearly parallel to each other—play a part in the operation. They are, indeed, much less developed in the female than in the male, but the male is so frequently found in the shaft that it seems but reasonable to suppose that he takes a share in the work of excavating.

Down these he will often see beetles quietly escaping. These holes are the burrows of different species of dung-beetle, into which they convey a quantity of excrement, and in it deposit their eggs. Here, again, as in the case of the sexton-beetle, is perfect scavenging. A large part of the dung—more than is merely required for the wants of the young grub—is carried down into the earth, where also the excrements of the maggot are retained and disinfected. In one single patch of dung, therefore, we may find examples both of perfect and of very imperfect scavenging; the former affected by different species of *Geotrupes*, *Aphodius*, *Typhœus*, &c., as just described, and the latter by *Staphylini*, Histeridæ, &c., which merely devour the dung where it lies, without carrying it down into the soil. Here then, again, the question may be asked—If to bury putrid matters be an instinct specially implanted in, *e.g.*, the Geotrupidæ, in order to preserve the atmosphere from taint, why has it not been extended to all the dung-feeding species? Even among those which do bury excrementitious matters we find many gradations in their mode of working. The *Geotrupes* and their near allies, in Britain and Central Europe, as we have seen, excavate a shaft directly under the dung, and carry down as much as they think requisite. The species of *Ateuchus*—the sacred beetle of the Egyptians—make up a ball of dung, in which they enclose an egg, and push or roll it along to a hole which they have either dug or selected with some little adaptation. *Pachysoma æsculapius* does not make up a ball, but deposits its egg in a hole, to which it brings dried dung in pieces. *Coprobius volvens*—the "tumble-dung" of North America—is a ball-roller. But among the *Coprobii* of Brazil one alone (*Coprobius carbonarius*) buries dung. These facts, if fairly weighed, seem to show that the practice of burying filth is not a primordial instinct, but has been acquired in the course of successive generations, and has taken various developments in various groups. We find, also, among these dung-buriers, some species which agree completely in their food and in their habits with the *Necrophori*. Whilst the other members of the great and splendid South-American genus *Phanæus* burrow under dung precisely like our British *Geotrupes*, *Phanæus melon* mines under dead fish, and *P. nigro-violaceus* and *sulcatus*—according to Prof. Westwood—dig holes beneath dead serpents, and bury them in a few hours.

We have next to point out an important shortcoming in the dung-eating, and even in the dung-burying, beetles. What the latter class undertake is done to perfection, but they decline some of the most important duties of a scavenger. They eagerly attack the dung of the ruminants and of the Equidæ. But such excrement, after all, consists chiefly of comminuted vegetable fibre, and—except artificially collected together in large quantities, where it may enter into fermentation or pollute rivers—it can scarcely be considered as markedly injurious to the health of man or of other animals. The dung of omnivorous beasts, as the hog, they do not affect; that of the Carnivora and of man they seem ordinarily to avoid. The *Hybosori* of Brazil, indeed, according to Westwood, frequent human excrement, but without burrowing in it, and a son of the writer once captured *Onthophagus nuchicornis* on the same material in Highgate Wood.

Generally speaking we are, then, warranted in concluding that the more offensive and dangerous any kind of excrement, the less are the Saprophagous Lamellicornes disposed to undertake its removal, leaving the worst cases to creatures who deal with them in a very unsatisfactory manner.

Passing in review the remaining "orders" of insects, we find no true scavenging species in the Lepidoptera. Some of the most beautiful butterflies will, however, occasionally indulge in a taste for abominations. *Apatura Iris* will come down from his airy flights to sip the putrid moisture oozing from a dead rat or weasel. The splendid *Papilios* and *Ornithopteras* of warmer climates will, in like manner, stoop to human excrement and carrion. This is a curious instance of animals purely herbivorous in their earlier stages becoming carnivorous, or rather omnivorous, when mature. The clothes'-moth and its congeners, indeed, feed upon dead animal matter, but only upon such as does not readily enter into decomposition and become offensive.

Among the Hymenoptera, the ants prey upon a great variety of substances, living and lifeless. They by no means refuse the bodies of small animals, birds, &c., which fall in their way, and may thus—by consuming matter that might otherwise be left to putrefy—rank as indirect scavengers. But we have never met with any authenticated case of their devouring matter already putrid, and certainly not excrement. Among ants we meet with the only established instance among the lower animals of a formal burial of the dead.* Wasps and hornets carry off fragments of meat from the butchers' shops, but they avoid carrion. Excrement they hold in great horror, and it is interesting to watch the nicety with which a wasp snatches away flies from such nuisances without soiling its feet or wings.

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* Journal of the Linnean Society, v. 217.

Among the Orthoptera, Neuroptera, Homoptera, and Hemiptera there are no true scavengers. The Diptera, on the other hand, feed to a very large extent upon carcases and fœcal matter, and many of them may at first sight be taken for scavengers of great efficiency. There are very few of the Muscidæ—the family including the common house-fly—which do not, at one part of their lives, feed upon dung, carrion, or decomposing vegetables. Thus the blow-flies, *Lucilia Cæsar* and *Calliphora vomitaria*, like the *Silphæ* and *Necrophori*, deposit their eggs in dead animal matter. But, unlike the beetles just mentioned, they neither bury the substances containing their eggs nor do they restrict their attacks to tainted meat. Their ova and larvæ possess a remarkable power of setting up and intensifying putrefaction in any animal matter with which they come in contact. Even living animals are not exempt from the attacks of these and allied species, which take every opportunity to lay their eggs in open wounds or abrasions of the skin. These flies, also, after being in contact with the most loathsome substances, settle upon man's person and on his food. Now how minute a portion of putrid blood, pus, &c., may set up dangerous changes in articles of diet, or may serve as the vehicle of disease, we do not exactly know, but we have reason to believe that exceedingly small quantities will suffice. Very similar charges must be brought against the house-fly (*Musca domestica*), the privy-fly (*Anthomyia canicularis*), and the blood-sucking *Stomoxys calcitrans*. All these, and many more indeed, at once consume nuisances and propagate them. The proboscis of a *Stomoxys* thrust into our flesh has perhaps, a moment before, been saturated with morbid matter, and the result of its bite may be carbuncle. Ophthalmia is certainly propagated by flies, and a closer examination will doubtless prove that variola, typhus, black vomit, scarlatina, and zymotic disease generally are spread about in the same way. Hence it is of the greatest importance that the dejections of patients suffering from such diseases, the bodies of the dead, and every substance which can have imbibed the morbific matter, should be treated in such a manner that these minute harpies may be prevented from settling and feasting upon it. The open cesspools attached to privies in country places are often one wriggling mass of maggots, and seem to call loudly for a liberal dose of carbolic sulphite, or some other enemy to low forms of life. It is in such localities that the attack must be made.

To destroy these pseudo-scavengers—who in reality, like the "pushing" quacks of whom they are the type, intensify the ills which they seem to cure—we must poison their pabulum. One of the perils of sewage irrigation is, that it gives great facilities for the multiplication of flies in the ever-moistened earth. The claims of the mosquito and its allies to be regarded as purifiers of the rivers, and of the house-fly to rank as the purifier of the air, we shall discuss below. Reviewing, then, the character of Dipterous dung and carrion devourers, we are tempted to ask—What is, after all, their especial mission? Are they adapted to remove putrid matter, or to propagate putrefaction and disease? We fear the balance of evidence is in favour of the latter view, and that one great class of nature's scavengers does more harm than good. The significance of this is apparent. But the most remarkable fact is that these pseudo-scavengers, the Diptera, are more successful than the genuine perfect scavengers among the Coleoptera. They are ten-fold, perhaps a hundred-fold, more numerous than the Silphidæ and the Saprophagous Lamellicornes. Instead of receding before the advance of civilisation and the increase of human population, they seem—like rats, mice, bugs, cockroaches, &c.—to grow upon us, and may yet constitute a danger more serious than some of us imagine.

Several Crustaceans will be mentioned among aquatic scavengers. The land-crabs, however, of which there are several species in warm climates, are given to prey upon dead animals. Some of them have been even known to devour human bodies which had been negligently buried.

The snails and slugs, to which we shall have to recur as consumers of vegetable refuse, occasionally prey upon dung. We have frequently, during entomological rambles in the early morning, seen the large common black slug feasting heartily upon human excrement. To what other species this habit extends we are unable to say, but we commend the fact to the careful consideration of all lovers of *escargots*.

Vegetable refuse probably contributes as much to the unhealthiness of a district as animal substances, being generally much more abundant. Its removal is very unequally provided for, and is scarcely even attempted by vertebrate animals. Decayed and decaying timber is broken up and consumed by myriads of insects: in cold climates by the larvæ of the goat-moth, the wood-leopard, the stag-beetle* and its congeners, the Longicornes, termed wood-beetles by pre-eminence, and many others: in warmer climates the task is taken up by the splendid Buprestidæ, the Dynastidæ (such as the elephant and Hercules beetles), and some species of the dung-burying tribes, as well as by the numerous and gigantic Longicornes of those regions, such as the "harlequin." The *Termites*, or white ants as they are improperly called, also engage in the task of removing decayed wood, but as they are equally prone to consume sound timber they must be regarded as scavengers of the lowest or objectionable type, like the carrion-flies.

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* We have never known a perfectly sound tree attacked by this fine insect (*Lucanus cervus*), and we therefore question the justice of ranking him among the enemies of the gardener.

Fallen leaves are not efficiently consumed by any class of animals, and where they have accumulated in quantities they may still be found in the next season in various stages of decomposition. Slugs and snails are considered as the scavengers for *effete* vegetable matter, but they occupy themselves chiefly with eating sound leaves and fruits, and hence they must rank in the lowest class. To some extent decayed leaves are pulled into the ground by earth-worms, and when far advanced in decomposition are attacked by the Brachelytra, and others of the many insects that help to dispose of the dung of herbivorous animals. But there is evidently no systematic removal at all commensurate with the occasion.

That the waters are polluted by their inmates, animal and vegetable, needs little formal proof. Their excrements, their cast-off skins, their decaying bodies, their abortive ova, all contribute a large supply of offensive and injurious matter. Nor are the dwellers on land excluded. From the autumn leaves which fall into the forest-pool,—from the decaying tree-trunk, fallen into the river, and gradually yielding up its soluble constituents or the products of its decomposition,—onward to the drainage of the cess-pool or the churchyard winning its way into the village well and to the sewage of some "closetted" town, contaminating the main arteries of a kingdom,—we find everywhere the same class of results. The waters are tainted by the residues of animal and vegetable life. The contamination is going on all around, but what are the means for its abatement? Has Dame Nature issued a "Rivers Pollution Commission," and, if so, is it more efficient than that lately sent out by our gracious Sovereign? Some self-styled authorities on this question fall little short of declaring that there is no natural process by which water, once contaminated with animal excrement or with putrid carcases, can be purified. With all cleansing operations, real or imaginary, carried on by lifeless agencies we have here no concern. There are certain water-plants which are powerful purifiers. Like all the higher forms of vegetable life they decompose carbonic acid under the influence of light, and evolve oxygen, by which the impurities are literally burnt up. Among such plants the common duckweed holds no undistinguished place.

Animal scavengers exist, also, which eat up the putrid or putrescent matter. Some work of this nature is done by aquatic birds—by none more eagerly than by the common duck. Any dead body floating in the water, whether that of a mammal, bird, or fish, is greedily gobbled up by these unclean feeders. We doubt whether any reptile or amphibian can be called a purifier of the water. The frog is commonly supposed to exercise this function, and its presence in wells is therefore viewed with approval. Now there is no doubt that it consumes the larvæ and ova of insects,—matter by no means desirable to be introduced into the human stomach, —and thus to some extent improves the water; but it seeks living prey, and we have never known it consume any dead or putrid matter, whether of animal or vegetable origin. Fishes—for example, the eel—and Crustaceans are diligent consumers of dead bodies floating in or sunk under water. The shrimp, by its performances of this kind, has earned the title of the "scavenger of the ocean." But we have no reason to believe that either fish or Crustacea can deal with those minute, almost pulpy, fragments of decomposing matter which form so large a proportion of the pollutions of lakes, rivers, and even seas. Here, as far at least as fresh water is concerned, the task is taken up by insects. It is well known that many Diptera and Neuroptera, though winged creatures, when arrived at maturity, begin their life in the waters. Many of these, when larvæ, feed upon this very pulp of decomposing animal and vegetable matter to which we have just referred. To this class belong the gnats and mosquitoes. It is accordingly maintained that the annoyance which they occasion when mature is compensated, and even outweighed, by their sanitary services when larvæ. Without their aid, it is said, certain districts in tropical climates would be absolutely uninhabitable on account of malaria. We must confess to no small amount of scepticism on this subject. We find, first, mosquitoes very prevalent where malaria is malignant. Here, then, is a proof that the mosquitoes, if they contribute anything to the health of a district, fall incalculably short of what is requisite. Again, we find them swarming in countries where malaria is unknown, *e.g.*, in Lapland, and where, from the nature of the climate, it seems not probable to arise. There are, also, districts in tropical South America exceptionally healthy, and, by a curious coincidence, free from mosquitoes and other insects of similar habits. Yet more, there are places —now and always healthy—where mosquitoes were formerly unknown, but where they have now been introduced, probably by shipping, and appear to be gaining ground. Surely these considerations go far to disprove the notion of mosquitoes being important sanitary agents, specially destined for the purification of pools and rivers. But there is yet a further objection, applying more or less to all natural scavengers which remove nuisances by using them as food. Granting that the mosquito in its larva state consumes a certain quantity of putrid or putrescent matter, there is a very considerable set-off. Its excrements, its cast-off skins, and finally its body when dead, are matter no less offensive and dangerous than the food which it has eaten. An insect when dead is, size for size, as decided a nuisance as the remains of a larger animal. That pestilence has followed the death and decomposition of large armies of locusts is a well-known fact. We cannot believe that the millions of dead mosquitoes can have any much more beneficial effect.

We are not aware of any Coleopterous, Hemipterous, Homopterous, Orthopterous, Hymenopterous, or Lepidopterous insect that—either in the larva state or after reaching maturity—takes part in the purification of waters. Many species of the two former orders are aquatic, but they select living prey.

Water-snails and other Mollusca may be ranked among the scavengers of the water, subject always to the limitation that they, in turn, produce a certain amount of pollution. They have, at any rate, this advantage over the mosquito and its Dipterous allies, that they do not issue from the waters and devote the rest of their lives to the annoyance of mankind.

But besides solid suspended nuisances, in a coarser or finer state of subdivision, polluted water contains a certain amount of organic matter in a state of actual solution, such as the extractive matter of plants and the fluid secretions and excretions of animals. This soluble impurity cannot be eaten out of the water like suspended matter, and the bulk of the above-mentioned agencies are, therefore, useless for its removal. Fresh-water Mollusca may, perhaps, purify such waters to a small extent, but the oxygen liberated from aquatic plants is far more widely efficacious.

But the worst remains to be told. So long as a stream of water is only moderately impure the plants and animals above mentioned exercise a very beneficial action upon its condition, and if they do not succeed in bringing it to a state suitable for domestic use, they, in millions of cases, prevent it from tainting the air. But it is said, and not without a foundation of truth, that if the pollution becomes excessive Nature's scavengers either beat an ignominious retreat or die at their posts. We must admit that there are in England, and probably in other populous countries, waters so contaminated as to be habitable by nothing higher than Fungi and Infusoria. A certain French physicist has even proposed the presence of certain species of plants and animals, as furnishing a scale by which the sanitary condition of a river might be approximately estimated. It may therefore be interesting to give a case in point, which was observed and carefully studied by the present writer in the spring and summer of 1868. With a view of throwing light upon a scheme in contemplation for purifying the refuse waters of a large dye-works, we visited a branch of the Calder and Hebble Navigation, which extends from the bottom of the town of Halifax down to the village of Salter Hebble, about 1½ miles off, where it joins the main trunk of the Calder. The descent is so steep that the canal is merely a series of pools separated by locks, and at its origin in the town it is fed not with water, but with sewage pumped up from the shallow River Hebble, which receives the domestic and manufacturing refuse of the town.* Having inspected the basin, where the water was very foul and turbid, and emitted a disgusting odour, we walked along the towing-path, carefully noting the phenomena presented. For the first two or three locks there was no material change; no animal life could be detected in the water, and no plants were seen except sewage fungus, which had here and there attached itself to the stonework. At last, when about half the descent had been accomplished, water-weeds began to make their appearance—at first few, and by no means flourishing. They rapidly became more numerous, both in individuals and in species, and more luxuriant. The water exhibited a corresponding improvement in colour and odour, and when we had arrived at the last pool—above Salter Hebble—we recognised a variety of water-insects. The common whirligig beetle (*Gyrinus natator*) was spinning round amidst the duckweed; an *Acilius* and several *Colymbetes* plunged into deep water as we approached, and the water-scorpion was disporting himself on the surface. None of these insects contribute at all to the purification of the water, being carnivorous; but their presence was a proof of the existence of other forms of insect-life, on which they could prey. The canal, it must be added, was very sparingly used, and its successive pools might be regarded as a series of subsidence tanks. Hence it appears that subsidence and exposure to air alone will bring excessively polluted water to a state which permits of the existence of aquatic vegetation and of insect-life. This point once reached, a further and rapid improvement is effected by these natural agencies. In a flowing stream, or in a canal at one uniform level and stirred up by constant traffic, these conditions would not occur. Under ordinary circumstances, therefore, it must be admitted that natural scavenging arrangements fail in case of excessively polluted waters. One error must be here carefully guarded against. Trees and shrubs growing in or near the water have no direct part in its purification. The oxygen given off from their leaves mixes with the atmosphere, and only acts upon the surface of the water; but plants whose leaves are on or underneath the surface give off oxygen, which in its nascent state comes in contact with the suspended or dissolved impurities, and effects their combustion.

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* We understand that these arrangements have been since altered.

We have lastly to turn to the pollutions of the atmosphere, and to inquire whether there are any aërial scavengers—any plants or animals engaged in the removal of volatile nuisances. That such nuisances exist cannot be doubted. All land-animals throw off from their skins* and from their respiratory apparatus a large amount of refuse, in the state of gases and vapours, probably also of solid matter in minute particles. The decomposing remains alike of plants and animals, together with the liquid and solid excrements of the latter, and, in short, all putrescent matters whatever, pollute the atmosphere, as is proved by the very fact of the evil odours they emit. Many of these impurities are dealt with by inorganic agencies with which we have here no concern. The carbonic acid breathed out by animals—which is, strictly speaking, a gaseous excrement—is removed by plants, which may thus claim a very prominent place among Nature's scavengers. But we have to examine, further, if there is any animal especially qualified to attack those solid impurities which modern research has detected in the air? To this question an answer has lately been returned in the affirmative, and the animal selected for this duty has been the common house-fly! We have no need to be surprised at such a choice in an age which has produced Kingsley's "Ode to the North-east Wind," and certain attempts to "rehabilitate" the characters of Robespierre and of Henry VIII. It was first asserted that the gambols of the fly in the air of a room were to be compared to the evolutions of the bat or the swallow; that it was, in fact, hawking for food, and that it caught on the wing and swallowed both any suspended particles of putrescent matter and those animal and vegetable germs which are now believed to play an important part in the propagation of zymotic disease. To this the reply is very simple and very conclusive. The mouths of creatures which hawk for food —such as the bat, the swallow, and the goatsucker—are characterised by their wide gape. They are open traps, which are borne rapidly against the intended prey, and at once close upon it. The mouth of the house-fly, on the contrary, is a narrow tube, admirably fitted for pumping or sucking up fluids or semi-fluids, but utterly unsuited for seizing small solid bodies suspended in the air. The evolutions of flies, therefore, are to be compared rather with the flight of the pigeon than with that of the swallow; they are probably undertaken merely as an amusement, and the occasional collisions between two flies do not arise from their both making a simultaneous swoop at the same germ, but are entirely sportive in their character.

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* An exception will probably exist in the case of such species as are covered with hard, corneous, or chitinous layers.

Scarcely, however, have the claims of the house-fly as an aërial scavenger been got rid of by this consideration, than they are re-introduced under a modified form. We read in Hardwicke's "Science Gossip" for October, 1875, that a chemist was struck with the fact that flies, after soaring about in the air for a time, alighted, and wiped their feet, wings, and bodies, with very great care. Observing the process under a microscope, he found that a number of minute germs adhered to the body and limbs of the fly, which it thus stripped off, and finally devoured. He next discovered that the flies in well-ventilated rooms were always lean, whilst those in ill-ventilated places were fat and well-nourished. Upon these observations he founds the practical recommendation that housewives should not set poisonous mixtures for flies, but should merely cover up carefully all articles of food, and seek to get rid of the visitors by careful ventilation. This discovery is brought under the public notice as a confirmation of the "pious adage that every thing is of some use."

Now, that flies are in the habit of cleansing their legs, wings, and bodies, is a very old fact. They perform the operation principally when they have been partaking of their ordinary food, which, as we hoped was known to all the world, consists of something much more substantial than microscopic germs. That among the matter adhering to their surface such germs may occasionally be found, we do not mean to deny. Flies, in all probability, have their parasites, and are known to be liable to disease depending on a fungoid development in their bodies. That they may attempt to free themselves from parasites, and from the spores or germs of this disease, is not at all unlikely; nor can we feel astonished that the filth—living or lifeless—collected from the surface of their bodies is ultimately swallowed. The same thing is, in fact, done by the cat and all other animals which cleanse themselves by licking. But to assert that what the fly thus collects in the operations of its toilet forms an essential part of its diet, upon which the question of its condition depends, is about as rational as to declare that a cat is kept plump by the lickings of its fur. We know the food of the fly: we see it settling upon every article of our diet, and upon a great many things which not even a Chinaman will eat, and, with its protruded haustellum, imbibing their fluid portions; we find it even sucking up the moisture of perspiration from our skins. According to our observation it frequents those places mainly where the good things of this life abound, quite irrespective of their sanitary condition. That, like most other insects, it does not love what are called "draughty" localities,—premises traversed by strong currents of wind,—is quite true. But let a room be perfectly ventilated, without a draught, and stock it with sugar, sweetmeats, and fruits, and you will find the flies cluster thick as leaves in Vallombrosa. On the other hand, we have noticed the absence—or at least the great rarity—of flies in rooms where no articles of human food were ever brought, even though the presence of atmospheric germs in an unusual degree was highly probable. To take one instance:—About eight years ago we became the inhabitant of a quaint and roomy old mansion in the North of England, at a mysteriously moderate rent. We soon found that the atmosphere—not merely of the house, but even of its terraced gardens—was at times unbearably foul, and that ventilating arrangements and disinfectants were alike powerless. The drainage from the cesspools of a group of houses situate on higher ground had soaked down through the shaly soil, and poisoned the ground beneath the foundations. Here, then, was air in which particles* of putrescent matter and germs might be expected to abound, and where flies—according to the theory we are discussing—should have been fat and plentiful. In the kitchen and the dining-room they were found in about their usual amount, but in the room which we had selected for our study, and where no articles of food were ever brought, they were strikingly rare.

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* It may perhaps be prudent to remind all whom it may concern that "particle" is not synonymous with atom, or even with molecule.

We happen, also, to know a chemical laboratory situate in the basement story of a large block of buildings. The sanitary arrangements of the premises are imperfect, and sewage gases often force their way up the sink in spite of traps. Yet flies are here very scarce. There is no draught to banish them, but there is nothing to gratify their well-known "sweet tooth," and consequently they keep away. It may be argued that the flies would be expelled by acid and corrosive vapours.* This, however, is out of the question, as all operations involving the liberation of such products are conducted in an evaporation-niche of the most approved design.

Again, we have every reason to suppose that the air of sewers, of covered manure-vaults, and of dirty neglected cellars, should be pre-eminently rich in floating germs or cells, and in particles of putrescent matter. Yet flies carefully avoid such localities. Indeed, to darken an ordinary apartment—a step certainly not calculated to improve its sanitary condition—is one of the most approved methods of banishing these intruders.

It may further be remarked that flies, when soaring about in a room, do not visit every part of the space as an animal might be expected to do which was collecting food. They hover backwards and forwards, or wheel round in a circular or elliptical orbit in some given spot, often selecting as their centre the bottom of some suspended object, such as a bird-cage, a gaselier, or the like. They rather avoid the corners of the room. All this is exceedingly unlike the movements

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Insects do not, as might be supposed, invariably shun localities where acid gases and vapours are evolved. The writer has seen the woodwork of old and leaky vitriol-chambers, softened by exposure to atmospheric influences, aided by the frequent escape of sulphurous acid, selected by the wasps and hornets of the neighbourhood as an excellent source of fibre for the paper-work of their nests. In a much-decayed beam of this kind were found a large number of *Coccinella 7-punctata*. In dye- and print-works, during the summer and autumn season, moths are frequently found drowned in bowls of acid solutions of tin. Gnats have been perceived dancing merrily, and to all appearance unharmed, in the orange vapours escaping from a shed in which nitrate of iron was being made. On the other hand, a shed, which had been selected for the preparation of solutions of tin, and which had evidently been the home of numerous spiders, displayed, after a few months, merely the tattered remains of old and untenanted cobwebs.

of the bat or the swallow, and as decidedly like the flight of the pigeon, undertaken merely for exercise or amusement.

We have also to point out the very peculiar statement that the flies in well-ventilated rooms are found to be lean. We have yet to learn that flies attach themselves permanently to certain rooms or houses. We see them flying in and out at doors and windows, always, if provisions are not plentiful, ready to wing their way to "fresh fields and pastures new." It must, likewise, not be forgotten that the house-fly is not the only insect given to hover about with no ostensible purpose save amusement. Who has not seen the gnats dancing on a fine evening, and generally in a very analogous manner, taking their centre from the top of some projecting object, such as the summit of a tree, the top of a chimney, or even a gate-post. Are these, too, capturing microscopic cells and germs, either with their mouths or by adhesion to their limbs? If so, they go to work in a very foolish manner. Were they to disperse, instead of collecting in thousands in one small spot of air, their probability of meeting with good sport would be much greater. The cockchaffer, also (*Melolontha maialis* s. *vulgaris*), hovers—or, as the Germans more happily say, "schwärmt"—in hundreds over the tops of trees in fine spring evenings. Is it cell-hunting? Alas! the farmers and gardeners know too well what is its diet!

From all these considerations we feel bound to declare that the claim of the house-fly to be admitted as an aërial scavenger is not made out, and that—regard being had to its known habits as a propagator of disease and carrier of septic matter—the advice to spare or protect it is most injudicious. The pious adage that everything is of some use may, perhaps, be accepted if qualified by the equally true statement that everything is of some detriment, and that the evil in many cases largely overbalances the good. So far as we are then aware, there is no animal engaged in the task of removing aëriform or volatile nuisances, or in clearing the air of minute suspended solids.

There is still a further objection to the view that flies become lean in well-ventilated rooms for lack of floating germs to feed upon. Prof. Tyndall finds that in air kept perfectly motionless all suspended solid matter is totally absent, and that in such air putrefaction does not occur. The more quiescent, therefore, the air of any apartment, the fewer suspended germs, cells, or spores will be present, and the leaner, instead of plumper, will be the flies!

Summing up the facts which we have thus briefly recorded, it appears that there are among animals three distinct classes or grades of scavengers:—those which bury refuse, whether for the food of themselves and their offspring, or simply to avoid a nuisance; those which destroy offal by devouring it, but do not taint other matters; and, lastly, those which devour putrid matter, but, being at the same time omnivorous and obtrusive, diffuse the germs of putrefaction and disease. Towards these three classes respectively reason demands that we should assume a very different attitude. The scavenging creatures of the first class we should cherish, defend, and seek to multiply. Those of the second, provided they have no especially dangerous properties, like the wolf or the hyæna, we may tolerate, and in scantily peopled and semi-civilised countries we may even protect. Thus, wherever sanitary appliances are imperfect, it is good policy to preserve vultures, carrion crows, &c., by legal enactments. Against the third class, which diffuse septic poisons, and which prey upon sound and useful matters, we must wage an unrelenting and systematic war.

We have observed that not every kind of nuisance finds, in the economy of Nature, some animal expressly adapted for its removal, especially in a perfect manner. There is no insect which buries human ordure, or that of the Carnivora. If, at least, such a process ever takes place it is rare and exceptional. The dissolved impurities of the water are not met, especially when excessive, and the solid impurities of the atmosphere seem equally overlooked. Further, we find one and the same nuisance attacked by scavengers of all the three classes. A dead bird may engage the attention at once of *Necrophori*, *Silphæ*, and blow-flies; or a piece of excrement be visited at once by Geotrupidæ, Brachelytra, and dung-flies. Now, what should we say of an army where part of the troops were equipped with breech-loaders, part with muzzle-loaders, and part with matchlocks, or bows and arrows? What would be our comments if the commanders of the army were most solicitous to keep up the number of the matchlock men, whilst allowing the regiments armed with the breech-loaders to decrease? Or what should we think of a carrier who should employ, between the same two points, goods-trains, stage-waggons, and pack-horses, giving constantly a larger and larger proportion of the traffic to the latter? Yet these two imaginary cases are exactly parallel to what we find in the disposal of offal. Nature's sanitary service does not form a well-organised system in which provision is made for every kind of nuisance, and where every task is committed to that creature only which is capable of executing it in the most perfect manner. On the contrary, we find important matters overlooked, comparative trifles meeting with a superabundance of attention, and the best sanitary agents elbowed out of the field by imperfect workers who succeed in virtue of their very shortcomings. Such a state of things is obviously incompatible with the old theory which taught that the various species inhabiting any particular country were especially adapted to its climate, soil, &c., and were, like the component parts of some exquisite machine, ordained each for the due discharge of some important function. But if, with the New School, we regard the Fauna and Flora of any country as consisting merely of such species as have hitherto been able to hold their ground in the struggle for existence, and who possibly, but quite incidentally, render to man—or to the world at large—benefits or injuries, all becomes clear and intelligible. Thus a candid consideration of Nature's scavengers supplies us with valuable evidence in favour of the doctrine of Evolution.

There is yet another consideration :—We have seen that there are animal forms depending for subsistence upon dead matter in every possible stage, from the scarcely cold carcase, or the fruit just fallen from its spray, on to *débris* in which scarcely any trace of organised structure can be recognised. Without such matter these animals, with their present habits and as now constituted, could not exist. The *Necrophorus*, as we now find him, implies small dead vertebrate animals, or possibly Mollusca; the Dynastidæ prove the existence of decaying trees, and the Geotrupidæ that of large herbivorous Mammalia. Supposing, therefore, that every animal has some especial and unalterable function for which its structure is a necessary adaptation, the scavengers of Nature cannot have made their appearance on the scene until those animal or vegetable species whose remains or excretions they were ordained to remove had been for some time in existence.* The carrion-feeders would have been in evil plight if created before the animal population of the world had become numerous, and deaths consequently were frequent. Hence, on the same supposition of special creation and of permanence of function, many of the higher animals must not have succeeded, but have preceded in their origin a multitude of lower forms.* But if we suppose that animals in the course of generations adapt themselves by degrees, alike in habits and structure, to varying conditions, these difficulties disappear. It is very conceivable that animal forms which at one time preyed upon living beings, or upon growing vegetables, may have gradually begun to subsist upon the dead remains of either (as we see some of the most decidedly carnivorous species do to some extent), and may thus have become refuse-eaters in the fullest sense of the word.

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* Except we adopt the grotesque hypothesis that the earth at its creation was stocked with decayed wood, leaf-mould, decomposing dung of sorts, and the bodies of dead animals—a supposition for which precedent could be found even at the present day.

III. THE NEWLY-DISCOVERED FORCE.†

By George M. Beard, A.M., M.D., New York.

SOME of the more important facts in regard to the newly-discovered force I have already several times briefly presented in the "New York Tribune," and other journals. In this paper I shall endeavour to systematise our present knowledge of this force, and with as much clearness as the nature of the subject will admit. It is all the more necessary to make this attempt, from the fact that in the letters to the papers, and in the reports of the lecture I gave on the subject, there were certain omissions and errors that were almost inevitable in the presentation of a new and difficult theme, and, furthermore, some of the more recent and important experiments have not yet been made public. I began to experiment with the new force as soon as the announcement of its discovery was made, and since that time have devoted to it many nights, and certain portions of my leisure hours by day. It is proper to state that I have studied the subject independently, suggesting and carrying out my own experiments, especially those of a physiological character, and have repeated the majority of those made by Mr. Edison. It is proper also to state that from the outset Mr. Edison and his assistant, Mr. Bachelor, have in every possible way, and with great enthusiasm and kindness, co-operated with me, freely contributing their apparatus, their time, and their labour. I am under obligations also to Prof. J. E. Smith, for kindly co-operating in some of the earlier experiments made at the establishment of Mr. Chester.

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* The same consideration applies with equal force to the Entozoa and to external parasites.

† Communicated by the author.

President Morton, of the Stevens Institute, Hoboken, also generously placed at our disposal, for experiment, the magnificent apparatus of that Institution, and personally aided us in some of the investigations.

Method of Obtaining the Force.

All that is necessary to generate the force is a galvanic current of considerable strength, interrupted by a telegrapher's key, and passing through a small coil of fine wire. In order to capture the force, a piece of iron or cadmium or carbon may be laid across the end of the coil, or within the coil if it be a single coil. To this piece of metal a wire of iron or copper may be attached, and by this conductor the force may be led off to the gas-pipe or any other earth-connection, or to a stove or stove-pipe, or, indeed, to anything that acts as a conductor. Mr. Edison is quite positive that cadmium gathers the force somewhat better than any other metal. I have not seen or made any experiments in the comparative conductibility of different metals.

A battery current of considerable strength is needed, 5, 10, 15, or 20 Bunsen's cells; the number varying with the coil of electro-magnet used, and with the size of the batteries, and with the strength of the solution. It is probable that the force is generated even when fewer cells are used, but not always in sufficient quantity to give a spark, and hence we have no way of studying it, or of demonstrating its existence. The spark has been obtained when but four or even two cells were used. The interruptions may be slow or rapid. When slow interruptions are used, it is found that the spark of the force appears only on the *opening* of the circuit.

The above is probably the simplest method of obtaining the force, and least liable to error, and is therefore better adapted for experiments. Instead of a single coil without a core, there may be double coils with cores, and across the ends of these a piece of iron or cadmium may be placed. Small spools of fine wire seem to be preferable to large spools of coarse wire, and magnets with large cores do not seem to develope the force—at least our experiments with very large magnets thus far have been failures. From the immense magnet in Stevens Institute, Hoboken, we could get nothing. A number of physicists, in different parts of the country, who have attempted to obtain the force from large magnets, or from magnets with large cores, have entirely failed. The very natural supposition that the larger the magnet the greater the amount of the force, is not sustained by experiment. The inconsistency is more apparent than actual, for in large magnets with large cores the electricity, it may be supposed, is converted into magnetism instead of this force. A certain amount of suddenness of interruption is necessary to the development of the force, and in the large magnet of Stevens Institute a number of seconds—about fifteen, according to our estimate—elapse, after the closing of the circuit, before the magnet reaches its maximum, while on opening the circuit the magnetism is undoubtedly retained in the magnet for some time.

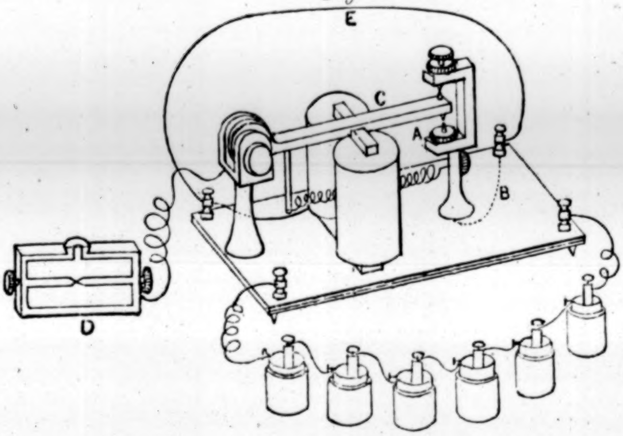
In the small double magnet usually employed by Mr. Edison the yoke is 2 inches long and 3-16ths of an inch thick. The two cores forming the magnet are 1½ inches long, each, and 3-8ths of an inch thick; the coils are composed of twelve layers of No. 23 insulated wire (Fig. 2).

Another method of obtaining the force is by means of self-vibrating electro-magnets of moderate size, the battery power remaining the same as by the previous method. In both methods the principle is the same, a strong interrupted galvanic current flowing through a small coil of moderately fine wire. In ordinary self-vibrating machines, used by physicians, the battery power is insufficient, and the core of the magnet is probably too large.

One of the most convenient methods for obtaining the force is from an ordinary telegraph "sounder" (Fig. 1). On most sounders the extremity of the lever plays between the points of two limiting screws. If the upper screw be insulated from the brass frame which holds it, and is connected to say 10 cells of bichromate of potash or Grove elements, the other end of which is connected to the lever of the sounder, the lever will at once be set in rapid vibration, like the magnetic interruption upon an induction-coil. The spark can be obtained by drawing the edge of a knife lightly across the top of the lever or from the metal at the base. If a wire be connected with one of the binding-screws the force will pass through it to any point, as the gas-pipe or dark box, where we can study it.

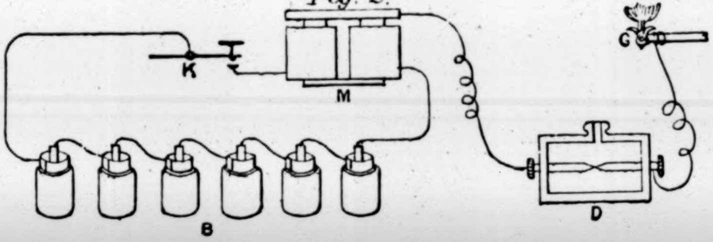
The force may be increased by uniting a number of the sounders connected either with separate batteries or with the same battery. When the force is thus increased, it may be obtained *without metallic contact* with any part of the self-vibrating apparatus: there appears to be a certain area or field in the air through which the force flies away from the electro-magnet, and in this field it may be captured by any

Fig 1.



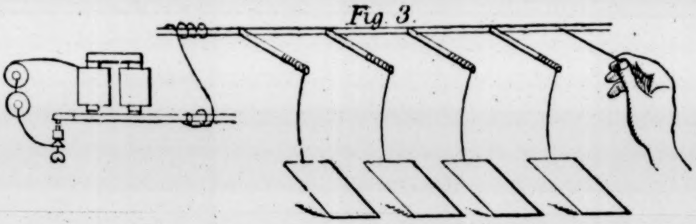
Force obtained from telegraph sounder converted into a self-vibrator.
C, Lever. A, Screw with platinum point, insulated by ring of ivory or rubber. D, Dark box, containing pencil points. E and B, Wires completing the connection.

Fig. 2.



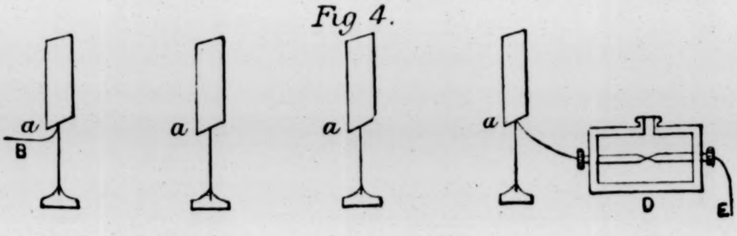
Force obtained from a small magnet, the interruption being made by a key.
M, Small double magnet. K, Key. D, Dark box. G, Gas-pipe or other earth-connection, or any large body of metal.

Fig. 3.



Drawing off the spark after a number of connections with the floor have been made.

Fig 4.



Force passing through the air.
B. Wire conducting-force. *a, a, a, a,* Large pieces of tin-foil suspended on stands. D, Dark box. E, Earth-connection.

metal of considerable surface that is brought near, even when it does not touch the apparatus.

These experiments, together with others to be subsequently described, make it probable that only a very small quantity of the force is captured by the conductors thus far employed, the greater quantity being diffused through the air. Hence I have made the suggestion, which in time will be carried out, to enclose a number of the vibrators with a metallic cone, covered with a layer of paraffin, which, according to my experiments, thoroughly resists this force, and connect the cone with wire similarly insulated.

The force can also be obtained from a Ruhmkorff's coil of moderate size, by drawing a knife across one of the posts near the end of the magnet. It does not seem to make much difference whether or not the outer coil be closed ; the force depends wholly on the primary coil. The difficulty of experimenting with Ruhmkorff's coil is that, unless great care be exercised, induction-currents will be obtained. In all these experiments, the battery, the electro-magnet, or coil, may be thoroughly insulated, so as to exclude all possibility of currents of induction over the sides of the cells, and the completion of a circuit through the air. The force can be obtained just as well, however, even when no pains are taken with the insulation. In studying this force the dark box is almost indispensable. A small box of any kind from which the light is excluded, except through a hole at the top, will answer. The pencil points should be carefully sharpened, and should meet in the dark box just beneath the hole in the top.

These pencil points should be made to approach and recede from each other, a distance about equal to the thickness of tissue paper, a number of times, when very weak sparks are to be detected, but when the sparks are strong they may be adjusted to be almost in contact, the fine particles of carbon forming a chain from one pencil to the other. In careful experimenting when the force is passing, or trying to pass, through great resistance, it may be necessary to watch for the spark a number of minutes, varying all the while the adjustment of the pencil points. On the gas-pipe, or stove, or rusty iron of any kind, the spark can be obtained so as to be seen in daylight, provided it be somewhat shaded ; on smooth metal it is difficult to get the spark.

Causes of Failure in Attempting to Obtain the Force.

Those who attempt these experiments for the first time may fail to see the spark from any of the following causes :—

1. The battery power may be insufficient.
2. The electro-magnet may be too large, or the core may be too large, or the wire may not be sufficiently fine.
3. Sufficient pains may not have been taken to darken the room. The strong sparks can be seen in full light of gas or day, but feeble sparks can be detected only in moderate darkness. The spark of this force is always comparatively weak.

A micrometer screw is of advantage in making the adjustment of the pencil points in the dark box. In the dark box a spark can be seen and studied when it cannot be readily seen outside. Very small iron wire and rusty tools give the spark better than copper wire or polished metals of any kind, the oxide of iron giving a more brilliant spark.

4. The person making the experiment may have connection with the conductor, and thus draw off a part of the force before it reaches the dark box or other point where it is studied. The body is a good conductor, and error from this source must be constantly guarded against.

Physical Experiments.

Out of a very large number of other experiments, I may mention the following:—I stuck a penknife in a large block of paraffin, and connected it with metal conducting the force, as a gas-pipe or wire, drawing the blade lightly over the conductor. No sparks appeared. When a long file was substituted for the knife, sparks were abundant, and were kept up as long as the connection was made. In these experiments the force appears to pass into the metal, and thence is diffused into the air. I suspended by long pieces of silk rolls of wire of various sizes, and allowed them to strike against the connection. With small coils sparks rarely appeared; with the larger coils they were abundant. It would seem, therefore, that a certain size is necessary in the conductor in order to get the sparks. A short bit of wire wound round a glass rod, and held against the conductor, would not get any spark; but take the same bit of wire connected at the other end with a spool of wire, or any large or long metallic surface, and the spark at once appears. A large surface of metal seems to attract the force better than a small surface. For this reason it is an advantage to connect the distal pencil point in the end of the dark box with the gas-pipe or other large metallic conductor: it is not, however, necessary to do so, for the spark will appear when the lead pencil is isolated. At one time I led the wire through a large vessel filled with water, and pieces of iron and bars of iron of various sizes were placed across its track and rested upon the wire, and the wire was wound round an iron press, and yet at the end the spark appeared. Mr. Edison took the wire out of doors, ran it along the ground and in a ditch on a rainy night, and brought it up-stairs several rods from the battery, and the spark was seen by him, by his assistant, and by myself in the dark box above described, but it was not constant, and required a nice adjustment of the carbon points to bring it out. The force, therefore, does not readily leave the metallic conductor, even when in contact with the earth or passing through water, and the spark is seen when the end of the wire conducting the force is at a long distance from the battery.

Physiological Effects.

1. The force is conducted by the human body. This was proved by taking hold of the conductor—a wire, iron bar, or gas-pipe—that was in connection with the apparatus, evolving the force by one hand, and with the other touching the blade of a knife to a stove or block of metal: sparks appeared, though somewhat smaller than when the force did not traverse the body. In some of these experiments, which were tried on several individuals, the body was insulated by a large block of paraffin 6 inches thick. When the distance for the force to travel through the body was reduced one-half—by making the connection at the back of the neck or in the mouth—a somewhat larger spark was produced than when the whole resistance of the body from hand to hand was included. It was clear, therefore, that the body conducted the force, though not so well as metals. A person standing on an insulator, with the conductor in hand, does not, on dropping the conductor, give any evidence of being charged; he can give no sparks to any other person or to any metal. I have also found it impossible to charge metals.

2. The force in passing through the body produces no demonstrable physiological effects. While we have the evidence of the sparks that the force is traversing the body, yet, wherever directed, it causes no sensation, not even on the tip of the tongue, no muscular contraction anywhere, no tremor, no erection of the hair, no flashes of light, no sour taste, no dizziness—in short, none of the usual physiological reactions of the different forms of electricity. Mr. Edison had supposed that in his own case contraction of the muscles

of the tongue was produced when he applied the tip to the conductor, and his head did really move up and down as though the muscles were affected, but upon my breaking the connection unknown to him his tongue kept moving as before, synchronously with the respiration. It was a case of mind acting on body; he expected some effect, and unconsciously produced it himself. Mr. Edison and two of his *collaborateurs* were taken sick in various ways one night, and it was supposed that the illnesses were caused by the force, but in this also they were probably mistaken; mind acting on body or coincidences may account for their symptoms. It is certain that I have experimented many hours by day and by night with this force, and a considerable portion of the time it was passing through me or into me, and I was not unfavourably affected, nor were any of those employed in the establishment, including Mr. Edison and the others who fancied their illness was caused by it. What effect the force evolved from a much more powerful apparatus, and passed through or into the body for a long time, may have, primarily or secondarily, I cannot say. Some who tested the matter thought that a very slight tingling sensation was experienced on the tongue, but closer examination did not confirm this.

3. The force, when generated in sufficient amount, causes the galvanoscopic frog to contract. It is well known that the irritability of frogs varies with the season of the year, and also with other conditions; hence the galvanoscopic frog cannot be an absolute standard or measure for electricity. For this reason, in all these experiments, the irritability of the frog was tested by a galvanic current passing through definite resistances. We tested the frog used in these experiments, and found it so sensitive that one electropoion cell, placed in a circuit having a resistance of 400,000 ohms, or nearly 35,000 miles of telegraph wire, caused contraction, and yet it did not contract when this force was passed through it. That the force in these experiments passed through the frog (which was insulated) was proved by the spark that appeared at the distal end. In this experiment, the result of which was most remarkable and unexpected, all conceivable elements of error seemed to be excluded. Subsequently—with a different apparatus, a Ruhmkorff's coil—a contraction of the muscles of the frog was obtained. The experiment was made at the establishment of Mr. Chester, and repeated in Newark.

In a subsequent series of experiments, made with Mr. Edison, contractions were obtained in the frog's leg, although the apparatus was most thoroughly insulated. As galvanoscopic frogs are susceptible to mechanical irritation, it was suggested that possibly the vibrations, from the apparatus communicated through the wire, caused the contractions, and on using the key (Fig. 2) instead of the self-vibrator the force caused no contractions. Returning to the self-vibrator (Fig. 1) contractions appeared. When the wire connecting the apparatus with the frog was shortened, the contractions increased in vigour. That the frog was susceptible to vibrations was shown by striking a very large tuning-fork and touching it to the sciatic nerve, and sometimes contractions appeared when the vibrating fork did not touch the nerve, but was held at a distance of ½ or ¾ of an inch from it. It was shown, however, by experiments on pith balls, that electricity is generated by a vibrating tuning-fork, and this electricity—which is probably the result of the impact of the steel against the air—may possibly have caused the contractions in the frog. On testing this frog by the galvanic current it was found that one electro-poion cell, after going through a resistance of over 1,000,000 ohms, or about 75,000 miles of telegraph wire, easily caused contraction.

In later experiments, however, made with Prof. Smith, I failed to cause any movement of the frog by mechanical vibrations alone, even when the nerve was held close to the self-vibrator; but when the force was allowed to pass through the nerve and muscles of the frog it contracted. When the key was used instead of the self-vibrator the frog did not move, even on the opening of the circuit. When the force coming from the self-vibrator was passed through several inches of water, so as to eliminate the error of mechanical vibrations, the muscles of the frog contracted. It is probable, therefore, that when developed rapidly and in large amount, as in the self-vibrator, this force causes contraction in the galvanoscopic frog.

The presence of electricity, in its different forms, is determined by the electroscope, the Leyden jar, the galvanometer, the electrometer; electrolysis, or electro-chemical decomposition, by physiological effects, and by light, heat, or ozone produced. This force, as thus far studied, does not deflect the leaves of the electroscope, nor charge the Leyden jar, nor move the needle of Thomson's delicate reflecting galvanometer or electrometer, nor decompose iodide of potassium, nor produce any demonstrable physiological effects on the nerves of motion or sensation, or speech, or on the muscles or other tissues; nor does it under all conditions always affect even the galvanoscopic frog, the most delicate of all tests of electricity.

On the different forms of calorimeter it has not been fully tried. I have convinced myself that, like electricity, it is resisted somewhat by platinum wire, and it is possible that in passing through platinum a portion of it may be converted into heat, as is the case with electricity; and if so a delicate thermometer, the bulb of which is surrounded by platinum wire through which the force is passing, would be affected. The heating power might be tested by the thermo-electric pile and galvanometer, or by the differential electro-calorimeter: experiments of this kind, however, whatever the results, would do but little toward solving the problem of the nature of the force. The odour of ozone, that is observed from the spark of dynamic electricity, I have not been able to obtain from this force.

It appears, then, that the light, as seen in the spark, and the contraction of the frog, are the only evidences we have of the presence of this force in any conductor. It is changed into light, as is electricity, in passing from one metallic conductor to another. This spark has yet to be exhaustively studied by the microscope and spectroscope. It is possible that it may affect some of the chemical substances chemically or thermically.

Non-Polarity of the Force.

The apparent non-polarity of the force appears in all its phenomena. Although, like light and heat, it may be capable of polarisation, yet, practically in the ordinary phenomena it is apolar, like those two forces, and as such it may be regarded. The idea of a circuit is not suggested by anything that is done with the force. We draw it off from the conductor as we draw off water from a spout, gas from a pipe, or heat from a stove. It has no tendency like statical electricity to distribute itself through the earth any more than any other conductor. When a direct passage to the earth or the walls of the room is established, by a gas-pipe, for example, it can still be drawn off from any branches of the pipe between the apparatus and the floor. It is captured by any good conductor, as metals or the human body, that is brought near to or in contact with a metal already conducting it.

With all the known forms of electricity, however they may differ in their special phenomena, the fact of polarity is inseparably associated; and it is by virtue of its polarity that electricity accomplishes work. Take away from the different forms of electricity their polarity, and you take all their practical usefulness in telegraphy, in electroplating, in signalling, in medicine, or in surgery.

Conceivable Sources of Error.

The presumption against the actual demonstration of the existence of a new force is very great, and can only be overcome by evidence of an overwhelming character. The experiments must be repeated with substantially similar results, at various times, and by different expert observers. When, however, the phenomena are admitted, and when it is admitted that they cannot be explained by laws of electricity as known to experts in that branch of science, the burden of proof is shifted, and the presumption is against the claim that these phenomena represent some known phase of the electrical force.

Mr. Edison and myself, and a number of physicists, who have thus far studied the subject, have made earnest and sustained efforts to prove that some known form of electricity would account for all these phenomena. Four theories of electricity have occurred to us, and by all of them these phenomena have been tested.

First. Creeping Electricity, which passes over the sides of the cells, and completes the circuit through the earth and air. This is the theory that would naturally occur to any physicist on first examining the phenomena. Besides the general fact that this force does not respond to the ordinary tests of induced electricity, this theory is met by two facts, either of which seems to be sufficient to overthrow it.

1. The phenomena of the force appear just as well when the cells of the battery and the entire apparatus are most thoroughly insulated. In one case the insulation was so complete that when the entire apparatus on the insulating stands were charged by statical electricity, the charge was retained for a long time, so that sparks could be taken from it; and yet the force appeared fully as strong as when there was no insulation.

2. In order to complete the circuit, it would be necessary for the induced electricity to traverse immense distances in the air, and at the same time have sufficient strength to give a decided spark. This is inconceivable; and, further, it is shown by direct experiment that this force, though it can go through the air when the surfaces at the ends of the conductors are sufficiently large, yet only for a few inches or a few feet at most, and when the ends of the conductors

are small wires the force will not pass—any long interval, at least—in sufficient quantity to produce a spark on the other side.

Secondly. The Extra Current under Ordinary Conditions of a Circuit.—It has been suggested that the force might be the extra induced current; but that current, as is well known, obeys all the laws of the other forms of induced electricity, produces decided chemical and physiological effects, causes a deflection in the needle of the galvanometer, everywhere gives constant evidence of polarity, and, so far as is known, cannot exist without a circuit. The spark of this force does not scintillate as much as the spark of the extra current. Nothing is easier than to prove the presence of the extra current; whatever this force may be, it cannot be that current in ordinary circuit.

Thirdly. Statical Electricity of High Tension.—High tension statical electricity, such as is obtained from statical machines, gives a jumping spark; its physiological effects are very powerful, and even dangerous; it can charge bodies and things; and when connected with the earth by a good earth-connection it at once disappears. The spark of this force is scintillating, not jumping, requiring light contact to obtain it. Examined under the microscope, even, it does not appear to jump any more than the spark of dynamical electricity. Moreover, this force has no demonstrable physiological effects, cannot charge persons or things, and when connected with the earth or floor by the best possible connection it can still be drawn off from the conductor.

Fourthly. Statical Electricity of Low Tension.—Low tension statical electricity obeys the laws of statical electricity of high tension in this respect—that it totally disappears when connected with the earth or with the walls of the room which are supposed to become polarised by it. Again, low tension statical electricity affects the electroscope and electrometer, and ought to charge the Leyden jar. Thomson's quadrant electrometer, which this force thus far has not affected, is a very delicate test for low tension statical electricity.

Mr. Edison has found that low tension electricity, such as is obtained from the free poles of a single galvanic cell, does not behave at all after the manner of this force. When the tension is increased up to sixty-five cells, and is further increased by employing a condenser composed of numerous sheets of tin-foil separated by paper dipped in paraffin, and having a capacity of 34 microfarads, yet when connected with the earth the electricity at once and entirely disappeared, and could not, like this force, be drawn off from the sides of the conductor.

Theories of the Force.

The theory which Mr. Edison favours is that this spark indicates a radically new force, to which he has given the provisional name "Etheric," from its tendency to diffuse itself in various directions through matter. This theory would regard this force as distinct from any form of electricity, as light or heat, and would indeed, bring it nearer to heat than to electricity, or would make it a kind of intermediate between those two forces. In the ultimate analysis there is probably but one force, of which light, heat, and the different kinds of electricity are modifications—modes of motion, differing in the nature of their vibrations, correlated to and capable of being transformed into each other. The old-fashioned fluid theories of electricity, although they still hold ground in school and college text-books, have been long since abandoned by physicists, and in their place the theory that it is a mode of motion of the ether, and of other matter through which it circulates, is more in harmony with recent science. Already several varieties of electricity have been discovered as follows :—

Frictional or statical electricity.

Dynamical or current electricity (including galvanic, induced, and thermo electricity).*

Induced electricity is farther subdivided into primary, extra, secondary, and tertiary currents, which differ from each other in quantity, tension, and in physiological effects. The tendency is for these different forms of electricity to approach to and actually run into each other.

An electro-magnetic apparatus is a reservoir of many forms of force—galvanic and induced electricity of various orders, magnetism, statical electricity, light, and heat. A source so rich in forces might give us at least one more; it is possible that one more has been here discovered. When a strong galvanic current, connected so as to flow through a coil of wire, is closed, a magnetic field is evolved, in and near the coil and at some distance from it, the electricity being converted into magnetism ; when the circuit is open, the electricity, it has been supposed, is converted into heat; it is probable that some of it is converted into this new force.

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* The statement recently made that thermo-electricity differs radically from any other form of electricity is incorrect. Thermo-electricity obeys the laws of dynamical electricity, however obtained.

In suggesting the theory that this force might be allied to electricity by supposing it to return, after the manner of the shuttle, to the source whence it is generated, I did not by any means commit myself to it; on the contrary, when all the known facts and phenomena that relate to the subject are carefully balanced, I find it as yet impossible to disprove the theory that *this is a radiant force, somewhere between light and heat on the one hand and magnetism and electricity on the other, with some of the features of all these forces.* But this claim is stupendous.

Experiments of the following kind are suggestive to enquirers in this department of research. When the wire conducting the force from the battery to the dark box is divided in the air, and the ends are separated even a sixteenth of an inch, no spark appears in the dark box. Lay these ends of the wire on a semi-conductor, as wood, and the force will pass even when they are separated a moderate distance. Place small pieces of tin-foil about these ends as they are again suspended in the air, and the force now passes an inch or perhaps several inches through the air. Place pieces of tin-foil of large surface about these ends, and the force will pass a longer distance. Make the surfaces of tin-foil larger still, until they are a foot square or more, and the force will travel several feet through the air.

Prepare three large pieces of tin-foil; place one piece at each end of the divided wire suspended as before, and the other piece about equal distance between them; and still the spark may be seen faintly, though irregularly, in the dark box. The force must go by induction or radiation from the piece of tin-foil to the middle piece, which acts as a kind of resting-place, and thence to the piece at the other end of the wire. The spark has been obtained, though with difficulty, and only after very nice adjustment of the pencil-points in the dark box after having passed through four pieces of tin-foil, the distance from the first to the last being *eight feet* (Fig. 4). The highest tension statical electricity, as generated by Holtz's machine, could not do this except by induction, and withal would require insulation.

When a number of Leyden jars are substituted for the pieces of tin-foil, the result is the same; but Leyden jars are insulated. In these experiments insulation is not required, as is shown by the following experiments:—

A large surface of tin-foil (6 × 6 or 12 × 12 ins.) was connected with one end of the divided wire and laid on a table. Over this were placed broad pieces of rubber, glass, or paraffin, and on the top of them was placed a similar piece of tin-foil connected with the other end of the divided wire, through which the force was conducted to the dark box. In this way I proved that the force could pass through 2¼ inches dry wood, two plates of glass each ¼ of an inch in thickness, ¼ of an inch of hard rubber, ¼ of an inch of solid paraffin, and 5 layers of paraffin paper. When the surfaces at the end of the wire were reduced in size, or when the tin-foil at one end was removed, the force passed less easily. When the tin-foil at both ends was removed, and only a few inches of fine wire constituted the surface, the force passed through only thinner resistances, and when *only the terminals* of the wire were applied to the surface of the resisting body the force would not pass at all, or but a very slight distance. The force passed through 20 inches of water in a small tube, and was apparently but little diminished even when the surface at the terminals was but the diameter of a small wire.

When a number of "sounders" are in action near together, and connected with the same or different batteries, the force can be captured by a piece of wire a few inches long, connected with the dark box, and held at a distance of several inches from any of the sounders. This is, however, no exception to the law that the force passes resistance by surfaces, for the sounders here constitute one surface and the few inches of wire the other, making a condenser or Leyden jar.

Some of the early experiments with this force gave erroneous or unsatisfactory results, because it was not known or suspected that with surfaces at the terminals it would pass through bad conductors even when the apparatus was not insulated.

In experimental researches we may learn oftentimes more from our blunders than from our successes. In studying the passage of this force through glass, the mistake was made of overlooking the conductibility of the air and the human body. On a long glass rod, held by a stand, were wound the terminals of the divided wire through which the force was passing. Before the rod stood one of the experimenters, placed his hands on the ends of the wires and pushed them an inch apart, and the spark appeared in the dark box. He pushed them several inches apart, still appeared the spark; yet farther, the entire length of the glass rod, 2 feet or more, yet the spark—though fainter—was easily seen. The inference, which for a number of days was unchallenged, was that the force passed through the long glass rod, though somewhat resisted by it. This inference was wrong. We afterwards found out, by a combination of accident with closer observation and a better knowledge of the conductibility of the air and the body, that the force in this experiment was all the while passing not through the glass rod at all, but through the body of the person adjusting the wires, or through the air from between the surface of his hand and the terminal; with one hand on one terminal and the other brought within a foot or more from the other terminal, the spark appeared.

Mr. Edison suspects, and he may be right, that in certain barometric and hydroscopic states of the atmosphere the force may pass through long rods of glass; but in the many experiments that I have made since the discovery of this error here noted, I have never been able to see the spark when any considerable length of glass or rubber or shellac was interposed between the terminals, excepting when there was at one or both of the terminals a large conducting surface.

Phenomena of this kind suggest magnetism more than inductive or dynamical electricity, but this force does not respond to the test of magnetism, the power to attract iron, and moreover exhibits phenomena that do not belong to magnetism. This force is attracted by iron and other metal as conductors, but it does not appear to attract iron.

The points which favour the theory that this force, whether electrical or radiant, is yet something new to science, may be thus recapitulated:—

1. It does not respond to any of the physical tests of electricity, except the spark.
2. It produces no perceptible or demonstrable physiological effects like electricity, save on the frog.
3. It gives no evidence, in any of its phenomena, of polarity.
4. It passes through the air and other resistances by large surface at the terminals, even when the apparatus is not insulated.
5. When connected with the earth or walls of the room it can yet be drawn off from the conductor.

Any known form of electricity giving a spark like the spark of this force would respond to some of the physical tests of electricity; would produce readily perceptible physiological effects; and would in its phenomena suggest polarity, even if rapidly reversed.

Again, the four facts regarded by me as favouring the theory that this force is allied to electricity are, when severely analysed, not so convincing as they might at first appear.

The spark of this force resembles the scintillating spark of dynamic electricity, so also do sparks produced by combustion. The velocity of this force is great, but so is that of light. This force is best conducted by metals, but so also is heat.

This force is resisted by non-conductors, but heat is similarly resisted, and both to a less degree than electricity. If it be as I have suggested, a form of electricity which is so rapidly reversed as to be practically depolarised, it would yet be electricity under very different conditions from those under which we are wont to consider it, and would be really a new force. The more I experiment in this department, and the more closely I reflect on the results of experiments, the farther I seem to be driven from the electrical toward the radiant theory of this force: in either case there would appear to be no ready escape from the acceptance of the conclusion that we have here something radically different from what has before been observed by science.

The relation of this force to the other forces may be thus represented:—

Light, Heat, . . New Force, ... Electricity, Magnetism.

The above would represent Mr. Edison's theory of a radiant force, nearer to light and heat than to electricity or magnetism.

The theory I have suggested would ally the force to electricity or magnetism more than to light and heat, as follows:—

Light, Heat, ... New Force, Electricity, Magnetism.

Presumption against the Validity of the Claim.

But it is yet too early to accept either theory. Although in the abstract there is no reason why a new force, or several new forces, might not exist in nature, the phenomena of which should be revealed by chance or otherwise, and which are now unknown and unsuspected, because we have no way of knowing the conditions necessary to produce them; yet practically, and in the concrete, the presumption against the validity of any claim to the discovery of a new force is in the first instance enormous, and can only be overcome by an enormous amount of expert evidence. Science it is said is sceptical; it ought to be sceptical. The sources of error in studying phenomena believed to be new are so numerous that the statements of the first experimenters cannot be accepted, and ought not to be accepted, until they have been confirmed again and again by competent experts.

Previous Attempts to find a New Force.

During the past forty years strenuous efforts to discover a new force have been made by scientific men all over the world. Mr. Edison himself, so he informs me, has made a series of elaborate experiments in this direction. In several instances men have fancied that success had crowned their efforts; hence have arisen the delusions of odic force, started by Reichenbach, with which this discovery of Mr. Edison has been absurdly confounded, and of mind-reading, or the power of conveying thought from one brain to another without the aid of the ordinary senses, as recently announced by some of the scientific professors of America.

For three reasons all these claims have been rejected by the scientific world. First, those who made them were not authorities on physiology, to which department their alleged forces belonged. Secondly, the experiments supposed to prove the existence of these forces, if we may accept the accounts given by the authors, were complicated with numerous and fatal elements of error, chiefly coincidences, mind acting on body, and trickery, any one of which would destroy the value of any scientific experiment. Thirdly, the alleged results have in no instance been satisfactorily confirmed by any expert in experimental physiology.

The claims of this newly-discovered force relate both to physics and physiology, and it is by physicists and physiologists, who are trained to habits of experimental research in their respective departments, that it should be studied.

The question whether there is in the human body any form or manifestation of force differing radically from those already known, I had already thoroughly investigated, and had reached long ago the decided conclusion that there was no evidence or suggestion of evidence of the existence of such a force.

Relation of Accidents to Discovery.

Into this and previous discoveries of a similar character the element of chance has largely entered; that is, fortunate accidents have occurred to experts capable of appreciating their meaning.

It was by accident that Galvani observed the twitching of the muscles of the frog, but for twenty years prior to that observation he had been studying the phenomena of electricity: it was by accident that Oersted discovered that a needle in the neighbourhood of a coil in which a galvanic current was circulating was put at right angles to the coil, but for fifteen years he had been seeking to accomplish this object. The spark of this new force coming from the core of a small magnet was accidentally discovered by Mr. Edison on the night of November 22nd, 1875; it has been often seen before by practical electricians and by others, and it had been assumed to be inductive electricity; it had been seen before by Mr. Edison, and had been unnoticed, but here, as everywhere, it was proved that the eye sees what it brings the means of seeing,—that it is not the eye, but the brain behind the eye that sees; for this time the spark was observed by one made specially alert by long practice in original experimental research in this department of enquiry.

If these experiments shall be so far confirmed as to become accepted by those physicists and physiologists who are competent to deal with questions of this nature, the discovery must assume great importance. It is forty-five years since any force has been introduced into science—the discovery of induced electricity by Faraday, dating from the year 1830. It is eighty-nine years since the discovery of galvanism; seventy-six years since the invention of the voltaic pile; one hundred and twenty-three years since Franklin, by his kite experiment, showed the identity of electricity and lightning; one hundred and thirty years since the discovery of the Leyden jar; and two hundred and seventy-five years since the publication of Dr. Gilbert's "Tractatus de Magnete," that marks the birth of the science of electricity.

Questions of Priority.

Close on the heels of every alleged discovery follow questions of priority. Already it has been claimed that the experiments of Riess, the great German authority on statical electricity, in obtaining weak sparks from the Leyden jar and Holtz machine, had really anticipated the experiments recorded in this paper. It is just to say that no such claim has been made or would be made by Riess himself, nor indeed by any one familiar with both series of experiments. The weak sparks of Riess were sparks of statical electricity; they exhibited polarity, and in other respects conformed to the laws of that form of electricity; he did not perform, and did not attempt to perform, with these sparks any of the physical or physiological experiments here recorded, nor did he claim to have discovered any new force or any new form of electricity.

The coincidence in time between the publication of the abstract of Riess's experiments and the announcement of the discovery of the new force is admitted, and if the series of experiments were identical there would perhaps be a just reason for raising the question of priority.*

Practical Value of the Force.

The practical value of this force it is yet too early to discuss. There is no evidence, as yet, that the spark can be seen after the force has traversed very long distances, beyond a quarter or half a mile. It is conceivable that it may yet be generated in sufficient quantity to send a spark through the outer wires of the Atlantic cable, and thus it may expedite ocean telegraphy; but there is as yet no proof that it can be utilised in that way or do any important work whatsoever.

Ii is entirely possible that it may produce important physiological effects, although such effects have not yet been demonstrated. If it be a radiant force, analogous to light and heat, it may, like those forces, affect the system radically, without giving rise to any sensation. Light is important, even indispensable for healthy life, yet its influence is never immediately or directly felt, save on the eye; and heat of a moderate intensity the body may receive unconsciously.

For the present, then, the question in regard to this force is primarily one of science, and it is to be studied by scientific more than by practical men, belonging rather to physicists and physiologists than to telegraphic engineers or physicians.

Time and many experiments may be needed before we shall be enabled to so increase and control this force as to make it supersede or supplement any of the ordinary uses of electricity. The history of electricity shows that long intervals often occur between scientific discoveries and their practical applications. Thus galvanism was discovered thirteen years before the invention of the voltaic pile; a number of years more passed before the pile was utilised in

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* The paper of Riess was published originally in "Poggendorff's Annalen," and the abstract referred to appeared in the "Electrical News," Nov. 15, 1875.

1807. Induction, discovered by Faraday in 1830, was not utilised in telegraphy for a long time, and was not formally introduced into medicine by Duchenne until after the lapse of seventeen years.

But even though this force shall never be used in the arts, or applied to any commercial purpose whatsoever, it must be, if its existence is established, of the highest scientific interest.

Every new fact or thought is, of itself, a positive addition to the world's wealth, and, however isolated at first, must in time affect both the past and the future, illumining the one and guiding the other.

Whatever the conclusion of physicists in regard to these alleged phenomena may ultimately be, the discussion of the subject cannot fail to be of interest and of value to the student of electrology, and for the reason mainly that it compels a re-investigation of the laws and phenomena of the different forms of electricity, in the light of the most recent theories of that force. A knowledge of electro-physics is indispensable to a thorough knowledge of electro-physiology or electro-therapeutics, medical or surgical: the value of many experiments in physiology, those of Ferrier for example, are seriously impaired for want of such knowledge. If it should be proved to the unanimous satisfaction of competent judges that these phenomena, so far as they are genuine, simply represent some well-known form of electricity, the labour and the time given to the subject will yet have been well spent.

For my own part, while I have all along inclined to the conservative view, that this spark indicates some form of electricity, and by that theory was first drawn to the study of it, I have yet been unable to prove this, or to obtain such proof from any source. Among the physicists who have given thought to the subject, and with whom I have conversed or corresponded, there is a wide diversity of opinion. Some men of ability and reputation have found great difficulty in getting the spark at all; others of equal ability and reputation have obtained the spark, and have satisfied themselves that it is merely the spark of the extra current of induction; others still are positive that it is statical electricity of low tension. No one, however, so far as I know, has published any facts that explain these phenomena by the unanimously admitted laws of any form of electricity; and no one, so far as I know, has yet repeated all the experiments here recorded.

Thus far those who are familiar with the *technique* of electro-physics, and who have the sounders and small magnets at hand, as telegraphers and telegraphic engineers, have been more successful in obtaining the spark, and are more nearly unanimous in the opinion that it represents something that cannot be explained by known laws of electricity, than professors of theoretical chemistry.

In conclusion, I may say that although I originally suggested the hypothesis that this force might be electricity so rapidly reversed as to be unable to respond to the usual tests, and therefore practically depolarised, and without a circuit in the ordinary sense of that term, and have kept that theory in mind in all my investigations, and hence would not unnaturally be pleased to have it proved to be the true solution of these phenomena, yet I cannot blind myself to these serious difficulties in the way of its acceptance:—

1. This force, when key is used (Fig. 2), and the interruptions are made slowly and carefully, appears only on the *opening* of the circuit. Where, then, is the opportunity for rapid reversals? Besides, induced electricity, even when very rapidly reversed, affects delicate galvanometers.

2. The physiological experiments are not fully accounted for by this theory. Any form of rapidly reversed electricity in an ordinary circuit, giving a spark like the spark of this force, is felt on the tongue and lips, and in the inner corner of the eye, and indeed on less sensitive parts of the body.

The galvanoscopic frog ought to contract even when the key is used with slow interruptions.

3. The passing of this force through great resistances of air, and solid non-conductors when large surfaces are at the terminals (Fig. 4). In these experiments these resistances may be supposed to act as dielectrics, and the whole arrangement may be regarded as analogous to a Leyden jar, or the condenser of a Ruhmkorff coil; but the extra current, as it passes through a condenser, is in a circuit: here there is no demonstrable circuit, and, furthermore, the dielectrics in the condenser are trifling in extent compared with the extent of air and thickness of resistances in these experiments. According to this hypothesis of rapidly reversed currents, induced electricity, when it reaches the large terminal, is changed into statical electricity, and at the distal large terminal is changed back into induced electricity, and through all these varying conditions the electricity is going backwards and forwards so rapidly as to be unable to respond to the usual tests. Is this demonstrable?

These objections may not be fatal to this working hypothesis of rapidly reversed electricity, but they must be met and fully considered by those who are inclined to favour it. It is the more important to insist on these difficulties, because a number of observers who have repeated some of these experiments, and admit the phenomena or some of them, are taking instant refuge in this theory as though that solved the whole mystery.*

Both theories, that of a radiant force and that of rapidly reversed electricity, are radically new suggestions, and neither of them can be, or will be, or ought to be accepted, except under the pressure of long-accumulating evidence.

Meanwhile it should be borne constantly in mind as a principle of evidence that for those who admit these phenomena, and who assert that they indicate some known phase of electricity or magnetism, the burden of proof in that issue rests to make good the claim by positive proofs. Until such proofs are obtained the scientific mind can maintain a position of neutrality, or can consider the relative value of the two theories here suggested. Either course would be consistent with the scientific spirit. The theory of rapidly reversed electricity apparently accounts for but a part of the phenomena, but it has this advantage over the radiant theory—that there is less presumption against it. If we could suppose a phase of statical electricity of low tension and considerable quantity, which, for some reason, is unable to respond to most of the electrical tests, these phenomena might perhaps be explained; but such a supposition really begs all the questions at issue.

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* Prof. Houston, of Philadelphia, among others, has repeated some of these physical experiments, and has adopted in full, after but a partial study of the subject, the hypothesis of rapidly reversed electricity as suggested in my letter to the "Tribune" of December 9th, and further claims priority of discovery, because he observed the spark of this when experimenting with a Ruhmkorff coil four years ago. To this claim, if it be seriously entertained, the obvious reply is that thousands of persons, probably, had seen this spark before it was *discovered* by Mr. Edison: it had been seen by Prof. Nipher, who supposed, and still supposes, it is the spark of the extra current; it has been seen by my friend, Prof. J. E. Smith, who assumed, as he tells me, without examination, that it was inductive electricity breaking through bad insulation; it had been seen, as has been stated, by Mr. Edison many times before he thought it worthy of study; it was undoubtedly seen by Prof. Houston, who, like so many others, failed to even suspect its meaning, and thus missed an important discovery. The honour of a scientific discovery belongs not to him who first sees a thing, but to him who first sees it with expert eyes; not to him, even, who drops an original suggestion, but to him who first makes that suggestion fruitful of results. If to see with the eyes a phenomenon is to discover the law of which that phenomenon is a part, then every school-boy who before the time of Newton ever saw an apple fall was a discoverer of the law of gravitation. Prof. Houston's account of his repetition of some of these experiments was published in the "Journal of the Franklin Institute" for January, 1876; and the article in which he incidentally mentions sparks that he now rightly recognises as the spark of this force was published in the same Journal, June, 1871.

IV. BIOLOGICAL CONTROVERSY AND ITS LAWS.*

MANY edifying commonplaces might doubtless be written on the intellectual fermentation, if it may not rather be called confusion, of the age. Nor can it be denied that tendencies supposed to have been long ago slain and sepulchred have risen again, and are asserting themselves with a hardihood which our fathers would have deemed impossible. When we find a scientific work—at any rate a work written by an eminent scientific man, and devoted to the discussion of scientific questions—formally dedicated to a dignitary of the Catholic Church as a vindicator of the rights of conscience (!), we may well ask, not jeeringly but sadly, " What is truth ? " We have witnessed of late brilliant progress in various departments of science; but we have also seen attacks made upon the very foundations of science. These onslaughts are increasing in frequency and in boldness. Metaphysicians and ecclesiastics are calling in question the inductive method, impugning the independence of Science, and seeking to re-assert over her the authority of " the Church." The battles of the sixteenth century seem about to be repeated. And some, who might claim to be the heirs of Galileo, think it no ignominy to wear the livery of Bellarmine and Caccini.

When we first opened the book which has suggested our present article we fully expected to find an intellectual treat of the highest order: its subject is one on which a most valuable work might well be written, and few living men indeed are better qualified to undertake such a task than is Mr. Mivart. Anti-Darwinian polemics we awaited, but such criticism, if conducted on legitimate—that is, on purely scientific—principles, we should be among the first to welcome, well knowing that in any issue Science must be the gainer. Although believing in Evolution, we have never given to the hypothesis commonly known as " Darwinism " more than a qualified and provisional adhesion. Whilst admitting that it has thrown a flood of light over some of the most difficult questions in Natural History, and has brought into vital connection a previously incoherent mob of facts, and that it is still a powerful and valuable instrument in the hands of the enquirer, we cannot forget that it has its difficulties. Some of these we have, on former occasions, endeavoured to point out. Hence we should cordially recognise any theory which should either supplement the doctrines of "Natural Selection" and "Sexual Selection," or modify them so as to get rid of their drawbacks and shortcomings. Nay, we should be well pleased to find them superseded altogether by a new hypothesis, adapted at once to the phenomena they have explained and the residues and anomalies which they have hitherto left unsolved. Such a hypothesis we thought Mr. Mivart might have produced, or at least have attempted; and the very attempt could scarcely be made, from a legitimate point of view, without leading to valuable results. Never were we more signally disappointed, although in these days the title of a book is often intended to conceal, rather than to reveal, its nature and object. The strange dedication was, in truth, but too ominous of the contents. The work we found was not constructive, but destructive. It consists of a series of attacks upon a number of men who have done good service in different branches of Science, such as Darwin, Wallace, Huxley, Tyndall, Galton, Lubbock, Helmholtz, Oscar Schmidt,—or who have dealt with methodology, such as Compte, Mill, Spencer, Lewes, &c. The doctrines of Natural Selection and Sexual Selection are indeed discussed, and a desperate effort is made to resuscitate the fast-fading notion of a "great gulf" between man and the lower animals. It is a curious fact that in the old Natural History man is supposed to hold, in relation to other animals, a place very similar to that assigned by the Lavoisierian Chemistry to oxygen in relation to the remaining elements. Unfortunately in biology, passion, prejudice, and sophistry play a more important part than they do in chemistry and physics. The discussion is based upon false principles. We all know the passage in which Mr. Wallace specifies the kind of controversy which alone can be recognised. "As his hypothesis is one which claims acceptance solely as explaining and connecting facts which exist in Nature, he expects facts alone to be brought to disprove it."* This method of discussion finds here comparatively little favour. Theories are tested by their supposed moral or religious bearings, or by their agreement with the author's *à priori* views. If we bring facts to prove the existence of reason in animals, we are told that we do not know what reason is; if we find in them evidences of moral life, it is said that we have "not even the faintest conception of what a moral nature is." If we show that they possess language, there follows the ready quirk that we confound emotional language with intellectual. That Mr. Mivart's own views of moral nature and of reason must be correct, no one, of course, is supposed to doubt; nor is the spirit of the argument sounder than its method. The author speaks, not as a judge calmly weighing the arguments on either side, and anxious merely that the truth should be ascertained, but as a passionate and eager prosecuting counsel, or rather as a *procureur du roi*, skilfully bringing forward every circumstance, every point—actual or inferred, relevant or irrelevant—which may in any wise damage the defendants, and with equal dexterity concealing whatsoever might tell in their favour. Deep personal hatred towards the "Agnostics" and their doctrines—the *odium theologicum* in its most malignant form—pervades the entire book. Mr. Mivart may doubtless be able to meet Mr. Darwin, Mr. Lewes, Mr. Spencer, or Dr. Huxley, on neutral ground or in private life, on terms of ordinary courtesy; but it is because the man is better and greater than his book. We find here nothing of that fine manly spirit expressed in the old adage—"Plato is my friend, but truth is more my friend." On the contrary, there is one passage in which Mr. Mivart almost seems to apologise for having, on some former occasion, spoken of Mr. Darwin with too much courtesy. For this he has now atoned to an extent almost ludicrous. We should not have felt in the least surprised had we found it proved—of course by strictly metaphysical arguments—that the author of the "Origin of Species" is the veritable transgressor who—

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* Lessons from Nature. By ST. GEORGE MIVART, F.R.S. London: Murray.

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* Contributions to the Theory of Natural Selection, p. 13.

> "Filled the butchers' shops with large blue flies,

or who—

> "With foul earthquakes ravaged the Caraccas,
> And raised the price of sugars and tobaccos."

Suppose, in all sober sadness, an enquirer knowing nothing more of Darwin than what he might learn out of "Lessons from Nature." Would he not go away with the impression that our great English naturalist had done little beyond launching a "puerile hypothesis," and had played a very unimportant—and, if anything, rather injurious—part in the development of biological science? Yet every candid critic must admit that, were the theory of Natural Selection superseded to-morrow, to Darwin would still belong the merit of effecting in Natural History a transformation as signal as that wrought in astronomy by Galileo, Copernicus, and Kepler, or in chemistry by Lavoisier; of bestowing upon zoology and botany a definite purpose and a direction for research such as before were wanting. His works would still remain a treasury of observations and of suggestions, and the impulse he has given to the Science would never die away. In England, Germany, America, naturalists have sprung up as if by magic in obedience to his spell, and Mr. Mivart himself can hardly be excluded from their number.

We need scarcely add that a critic unjust to persons will not be much more trustworthy as regards their discoveries and their doctrines. The evidence in favour of Natural Selection—and indeed of Evolution altogether—is strictly cumulative, and as such, whatever weight it may carry to the patient and dispassionate enquirer, it is peculiarly open to the attacks of an opponent at once skilful and unscrupulous. We do not, of course, mean to accuse Mr. Mivart of deliberate unscrupulousness. We all know the words —in themselves literally reeking with hypocrisy—in which "the Church" pronounced sentence of death on Giordano Bruno:—*Ut quam clementissime et citra sanguinis effusionem puniretur*." Yet even on that occasion we should be reluctant to declare that the judges were sinning against better light and knowledge. Just so here: Mr. Mivart doubtless believes and feels what he says, and considers his own line of criticism fair and honourable. We know that man is an adept in self-delusion, and of all men the metaphysician who has cultivated the art *s'égarer avec méthode* is most likely to go unconsciously astray.

We come now to a most painful subject, which, indeed, we would gladly pass over were not its consideration absolutely imperative. Mr. Mivart complains that in one particular instance Mr. Darwin departs from his ordinary courtesy to opponents. We are therefore justified in assuming that he regards courtesy to opponents as a duty—at least in others. Bearing in mind this circumstance we turn to page 144, and read:—" It is in one respect a calamity of our time and country that unbelievers, instead of, as in France, honestly avowing their sentiments, disguise them by studious reticence—as Mr. Darwin at first studiously disguised his views as to the bestiality (!) of man, and as the late Mr. Mill silently allowed himself to be represented to the public as a thorough believer in God." Along with this passage we take the remarks on "Mr. Winwood Reade, a friend and ardent disciple of Mr. Darwin," and on the teachings of "our English physical expositors" (pp. 393 to 395), and then ask whether the author is not, by implication at least, charging Mr. Darwin with atheism? This is the more probable as we can find no saving clause, or limitation guarding against such a construction being put upon these passages. Still, in a charge so grave the accused is entitled to the benefit of the faintest doubt, and Mr. Mivart may therefore claim a verdict of "Not proven." It is time, however, that we came to a full understanding about the foul practice of introducing charges of atheism in scientific controversy. On this subject we beg to offer the following considerations:—

(1.) Charges of "heresy," "infidelity," or "atheism" are beside the question. If a theory in astronomy, in geology, in physics, chemistry, or biology is in doubt, let it be judged on its own evidence; that is, let it be compared respectively with astronomical, geological, physical, chemical, or biological facts, and, according as it is able or unable to account for and to harmonise such, let it stand or fall. The man who is unable or unwilling to do this convicts himself, from an intellectual point of view, either of impotence or perversity, and should leave controversy to others.

(2.) Such charges, further, are delusive. Not to speak of the thoroughly trained scholar, even many of the "half-educated" know that almost every important discovery in Science has been denounced by the "*parti prêtre*" as impious, heretical, and atheistic. A yearly volume of the "Quarterly Journal of Science" would not contain the abuse uttered by ecclesiastics against the Copernican theory of the solar system, against the doctrine of a plurality of worlds, the Newtonian view of the universe, the nebular hypothesis, the chronology of modern geologists, &c. Yet all these views, and many more which might be mentioned, were found—when passion had cooled and sober judgment had time to decide—perfectly compatible, not with theism merely, but with Christian revelation. What "the Church" has cursed in one generation she "assimilates" in the next. What educated man, then, after reviewing the past, can dare to set aside modern theories in such a manner?

(3.) Such charges are, further, distinctly immoral, and even criminal. All civilised countries brand with ignominy the suitor or the advocate who suborns false witnesses, forges or destroys documents, or corrupts judges and juries. But the controversialist who charges his opponent with atheism stands in a precisely similar position. He well knows that although the public might not admit, *totidem verbis*, that "whatever an atheist advances must be false," or that "every theory once pronounced atheistic must be erroneous," yet it will practically act as if such propositions were established. Hence by making such charges he fraudulently attempts to steal from the public, through an appeal to their passions, a verdict which he has no hope of obtaining from their reason. Knowing and trading on the extreme animosity with which the heretic, the sceptic, and the atheist are —rightly or wrongly—regarded, he seeks to deprive his opponents of a fair hearing by applying to them these dreaded names. A meaner, a more infamous, stratagem can scarcely be conceived. Yet more: it is not the man conscious of the goodness of his cause who fights with such weapons. He who knows that his views are in harmony with facts has nothing to gain by foul play; but if he feels inward misgivings concerning the doctrines which he advocates, or doubts at least the possibility of bringing forward valid arguments in their defence, he may readily, if dishonest enough, seek to blacken the character of an opponent.

We may, therefore, safely and fairly conclude that whosoever in scientific controversy introduces accusations of atheism is, if not knowingly and wilfully, still decidedly in the wrong. We are consequently fully justified in shutting his book, and giving judgment against him.

But there is another consideration which here forces itself upon our attention. All writings calculated to bring a man into general "ridicule, hatred, or contempt," are by the law declared to be libellous. Now it is very questionable if, in England, any accusation is so much calculated to bring a man into "hatred and contempt" as a charge of atheism or "materialism," however ill-founded it may be. Surely therefore such charges, whether brought directly or by implication, are libellous, and as such they are more fitted to be dealt with by a criminal court than by reviewers. We should like to see such a case decided, and we believe that the result would be a great improvement in the tone of scientific and semi-scientific controversy.

But even if such accusations should be pronounced not libellous, and if those who resort to them have no legal penalties to dread, there is another tribunal which might interfere. Why should not scientific men, scientific societies, and scientific journals, agree that whosoever in a scientific controversy attempts to get rid of an opponent by raising the cry of atheism should be held to be *ipse facto* an outlaw, and to be no longer entitled to the treatment of a gentleman and a scholar? Nay, why should not other charges affecting the personal character of an opponent be dealt with in a similar manner? We do not, of course, seek to screen the man who can be proved to have suppressed documents, cooked results, or claimed as his own discoveries those which he well knew belonged to another. We refer to those random charges of dishonesty and mendacity, and those sweeping ascriptions of motive, which are unfortunately so common. Thus we have often heard and seen it asserted that the authors of some particular theory were actuated by a desire to disprove the existence of a God, to subvert the Christian religion or some particular form of it, or to injure public morals. To such assertors we would reply—"Prove your charge by evidence such as would satisfy an impartial court of justice, or take the consequences, which will not be pleasant!" We are here reminded that in the very passage in Mr. Mivart's book (p. 144) in which he comes unpleasantly near charging Mr. Darwin with atheism, he brings forward against the same gentleman something very like an accusation of dishonesty. It is perfectly true that in the "Origin of Species" Mr. Darwin does not pronounce as to whether mankind had or had not been gradually evolved from some lower form of animal life. But reticence is very different from dishonesty. A thinker is not absolutely bound to bring his speculations to light at all; for keeping them back whilst he is accumulating and weighing the evidence for and against them, he deserves praise rather than censure. Nay, even for introducing doctrines gradually, as the public are able to bear them, there is certainly authority which Mr. Mivart cannot consistently impugn. Nor must we forget that Mr. Darwin has, from the first, nowise courted publicity for his views. But for the fact that Mr. Wallace was known to be preparing a work of a somewhat similar nature, even the "Origin of Species" might never have seen the light.

There may be persons who will be aggrieved at this expression of our views on the subject of scientific controversies; but if they feel themselves guiltless they may cheerfully exclaim—"Let the galled jade wince." As for those who have actually made the kind of charges we protest against, they have no claim to lenity or forbearance.

Controversies on theories in the various inorganic sciences have been carried on with no little acrimony. But charges of atheism are, at least, banished. Why may not this reform be extended to biology and psychology? Those who cannot treat these subjects from a purely scientific point of view may serve to test the patience of unfortunate reviewers, but they cannot lead us to the truth.

Let us now return to the subject-matter of the controversy before us;—In one passage we find it asserted that the Darwinian theories have met with wide-spread acceptance among the "half-educated." This is quite contrary to our own observation. The most numerous and most virulent opponents of the doctrine of Natural Selection, and indeed of Evolution altogether, are to be found among the following classes:—Retail tradesmen, clerks, shopmen, commercial travellers, "smart" writers in the political press and in purely literary organs, Sunday-school teachers, ministers of the less intellectual dissenting communities, and clergymen who have not had the advantage of a university training. On the other hand, its popularity among working naturalists —" Maenner vom Fach"—is great, and that in proportion as they are working naturalists, men accustomed to deal with things rather than with words or with dreams.

Among the weapons employed against Darwinism a prominent place belongs to the admissions of its author and supporters. But these are almost invariably magnified and distorted, as is often the case, with isolated passages taken out of their connection. If an enquirer avows that his system needs modification, it by no means follows that he abandons it altogether, in any other sense than as we abandon a tentative hypothesis in favour of a closer approximation to the truth. Of the ingenious rather than ingenuous style in which the writings of Evolutionists in general, and of Darwinians in particular, are travestied, we cite the following as a typical instance:—Mr. Darwin having remarked that if man had not been his own classifier the notion of founding a separate order for his own reception would never have arisen, the comment is added;—"That is to say, the irrational classifier would necessarily have excluded the unknown element of reason as a basis of classification!" Mr. Darwin never suggested the possibility of such a contradiction as an "irrational classifier," but assumed animals to be surveyed by a hypothetical being higher than man, or at least totally distinct from him and free from his prepossessions. This "bull"—more absurd, if less humorous, than those of Irish origin—is from "Caliban; the Missing Link," a work written not by Caliban, but by a Mr. D. Watson.

Of course the most satisfactory manner of refuting the doctrine of Natural Selection must be to supersede it by some better hypothesis, just as the "emission theory" of light was refuted by the production of the "undulatory theory." What, therefore, does Mr. Mivart bring forward to account for the genesis of species? We will take his own words:—"It is quite conceivable that the material organic world may be so constituted that the simultaneous action upon it of all known forces—mechanical, physical, chemical, magnetic, terrestrial, and cosmical,* together with other as yet unknown forces—which probably exist, may result in changes which are harmonious and symmetrical; just as the internal nature of vibrating plates causes particles of sand scattered over them to assume definite and symmetrical figures when made to oscillate in different ways by the bow of a violin being drawn along their edges. The results of these combined internal powers and external influences might be represented under the symbols of complex series of vibrations (analogous to those of sound or light), forming a most complex harmony or a display of most varied colours. In such a way the reparation of local injuries might be symbolised as a filling up and completion of an interrupted rhythm. Thus, also, monstrous aberrations from typical structure might correspond to a discord, and sterility from crossing with the darkness resulting from the interference of waves of light.

"Such symbolism will harmonise with the peculiar reproduction of heads in the body of certain annelids, with the facts of serial homology as well as those of bilateral and vertical symmetry. Also, as the atoms (?) of a resonant body may be made to give out sound by the juxtaposition of a tuning-fork, so it is conceivable that the physiological units of a living organism may be so influenced by surrounding conditions (organic and other) that the accumulation of these conditions may upset the previous rhythm of such units, producing modifications in them,—a fresh chord in the harmony of Nature,—a new species.

Elsewhere he informs us that species arise in virtue of an "internal force or tendency," manifesting themselves "with suddenness, and by modifications appearing at once."

Mr. Mivart therefore does not, with Cuvier and the orthodox naturalists of the old school, maintain that every kind of animal and plant has been separately formed by a distinct act of Divine intervention, and endowed once for all with its present form, powers, and habits, and has been allotted to some particular district, there to exercise a given function for which it is especially adapted. He is therefore an Evolutionist as decidedly as Lamarck or Darwin, and is necessarily at issue with all who oppose the doctrine of Evolution *in toto*.

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* It is interesting to note the case of "cross division" presented to the reader in this enumeration of forces.

Whilst holding that species are mutable, he contends that their changes are not necessarily and invariably gradual, but may have been sudden. Borrowing the terms from geology, he is not a "uniformitarian," but a "catastrophist." The cause of such changes he considers to be not "natural selection," a hypothesis which he dismisses as puerile; not sexual selection; not the influence of changing climate, diet, and other external causes; not to the efforts of animals to adapt themselves to modified circumstances; but to a complex of agencies, internal and external, which might almost be designated "things in general," and of which the author himself, being only able to shadow forth his meaning in metaphorical language, has not, probably, the most distinct conception.

The first objection to Mr. Mivart's views is one which has often been urged against Evolution in general, but which is exceptionally formidable to the theory of sudden modifications. The champions of the Old Zoology are accustomed to say that no change of species has ever yet been actually observed; that animals constantly give birth to young in their own likeness; and that, arguing from the known to the unknown, such must have been the case from the creation of the world, or at least from the dawn of the present order of things, whatever that may mean. To this objection Darwin and Wallace, and all who hold that the difference between species and species has been produced by gradual divergence, have a ready answer. "The variation," they may say, "visible in the life-time of an observer is so trifling as to escape notice." To borrow an illustration from the author of the "Vestiges," as well might an ephemeron deny the development of the frog from the tadpole state, because during his life-time and within the range of the traditions of his ancestors the tadpoles in the pool had remained tailed creatures, breathing through gills. But such a reply is scarcely possible for Mr. Mivart. The appearance of a new mammal, bird, reptile, perhaps we may even add insect, would at once attract attention in any civilised country, often even among barbarians. Can Mr. Mivart adduce an instance in point? We know that species are continually discovered which are altogether new to Science. But such discoveries are most plentiful—

a. In countries imperfectly explored, becoming rarer and rarer as any region has been more fully explored by naturalists.

b. In the lower forms of animal life, and especially in very minute species.

In England the discovery of a new beetle an inch in length, or of a butterfly the size of *Vanessa Io,* and not obviously imported from some other part of the world, causes no little sensation. On the Continent the occurrence of a nondescript bird or reptile would certainly not be passed over as an every-day affair. Even in India, a buffalo, a deer, or a cat, unknown alike to native and British sportsmen, would excite astonishment. But if such sudden modifications ever have taken place, is it not likely that they would—occasionally at least—still occur, and that they would not be exclusively confined to imperfectly known countries, to microscopic species, and to the lower groups of the animal kingdom? Perhaps Mr. Mivart may say that the "internal forces or tendencies" of species and the external circumstances under which they are placed, have already reacted upon each other, and that no further changes are now possible. We reply that external circumstances continue to alter, and that, consequently, if a perfect equilibrium was at one time attained, the conditions under which it exists being no longer the same, it is liable to be disturbed, thus necessitating on his hypothesis fresh changes. The production of some authenticated case of a new animal or vegetable form evolved out of an old or known one, unessential for Mr. Darwin, is for Mr. Mivart an absolute necessity.

The illustration in which the new hypothesis is conveyed makes, after all, very little room for inward tendencies. The sand, or other powder in which the sound-figures are embodied, lends itself with the same facility to one kind of vibrations as another. The plate, its supports, and the violin-bow are all outward circumstances acting upon the sand. Thus the entire illustration is one which might have been very appropriately used by Lamarck, and in so far forth as it is fully and fairly herein expressed "Mivartism" is merely Lamarckism under a new terminology. Lamarck makes, indeed, no explicit reference to the internal nature of animals; but he must have implicitly assumed it, otherwise there would have been nothing upon which outward circumstances or forces might react. One distinction is, however, that Lamarck, like Darwin, supposed the variation of species to be gradual, whilst in Mr. Mivart's opinion it may be sudden. But are sudden changes of climate or other outward circumstances sudden? As to the latent internal tendencies they seem to involve greater difficulties and a more frequent recurrence to miracle than the old hypothesis of special creation.

But passing over these minor difficulties we come to the main question—the working of the hypothesis. We have before us certain phenomena, facts, and their relations. A new theory is placed in our hands: how far does it accommodate itself to phenomena? Can we show that it explains what we actually find, whilst if the facts were different they would clash with the theory? Does it give us any hints into what channel we are to direct our observations? Scarcely; it lays before us two unknown powers,—the internal tendencies and the complex of external influences,—and bids us from these deduce the animal kingdom. How are we to discover the magnitude, the direction, the *modus operandi* of either, much less mutual reactions? Surely such a theory is too accommodating, and would lend itself as readily to the monsters of heraldry and the phantoms of mythology as to animals that ever have existed.

Let us once more take Mr. Mivart at his own words, or rather at his own illustration. On the glass disc, then, lie the sound-figures traced in sand, resulting from the last application of the violin-bow. Let it be now applied in a different manner. Instantly, not one, not some, but all of the figures are altered. Translating the symbol into the thing symbolised, this would mean that in a certain organic species—say a butterfly—all the eggs deposited after the new external influences had come into play would yield insects not slightly but abruptly modified, and the old form in a few weeks, or at most months, would entirely disappear. So far this would suit Mr. Mivart perfectly, dissenting as he does from the old maxim that *Natura facit nihil per saltum*, for which he would substitute "*facit multa*," if not "*omnia*." But how does it agree with facts? On this supposition the rise of a new species would always be attended by the extinction of an old one. Never would a species branch out into two or more, nor would the old form survive the appearance of the new, save in some region to which the modifying influences might not have extended. Thus on Mr. Mivart's principle the multiplication of species, if it took place at all, would be exceedingly slow, and there could be no branching out into a number of closely approximating forms.

On the hypothesis of Natural Selection the process would be very different, and we think more accordant with observation. Suppose a new enemy makes its appearance in the country inhabited by our butterfly before mentioned. One modification of the original stock might escape with relative impunity by reason of superior swiftness; another by being of a shade less easily discerned, or by simulating some more formidable creature; another, perhaps, by being of an evil odour. Thus several species would branch out in different directions, whilst the original type might still exist for some time in gradually decreasing numbers. Thus in one of the very few cases where this Proteus of Mr. Mivart's can be fairly bound, and forced to give a definite reply, the oracle is not in accordance with facts. Whilst, therefore, we confess that Natural Selection has robbed us of no little of the pleasure with which we used to contemplate the animal and the vegetable world, and gladly as we should see it superseded, we cannot pronounce Mr. Mivart's attempt successful, and we doubt whether he is working in the right direction. In any case a vast amount of work requires to be done before his theory can admit even of precise verification. Might we suggest that such work would be infinitely more useful than the metaphysical warfare in which he is now engaged, and would far better merit the title of Lessons from Nature?

The most unsatisfactory, and at the same time the most painfully instructive, portion of the whole work is the attempted demonstration of a "great gulf" between man and the rest of the animal kingdom. The difference he considers as one not of degree, but of kind. Hence he is at issue not merely with Mr. Darwin and the more thorough-going Evolutionist of his immediate school, but with many naturalists who totally reject Evolution. Before Mr. Darwin was known, save as the author of the charming "Voyage of a Naturalist," we had carefully examined the respective position of man and "brutes," and had come to the conclusion that the vulgar doctrine of a great gulf, of a distinction *toto cœlo*, was utterly untenable. We saw that it was one of the lurking remnants of a vicious system of classification which has survived here longer than in other spheres of enquiry, because it panders to man's egotism and vanity. Since then we have met with no facts, no arguments, calculated to subvert our views, but with many, both facts and arguments, by which they are corroborated. It is our full conviction that Mr. Mivart's attempt is a signal failure. His position may be said most nearly to approach that of Swainson; but the great Quinarian excluded man altogether from the zoological circle proclaiming his structural resemblance to the apes,—relations not of affinity, but merely of analogy, and consequently of no value in determining his rank in the scale of Nature. To him man was not the highest animal, not "an animal and something more," but the lowest, aberrant, member of the spiritual kingdom. Such a doctrine might be hard to substantiate, but it was no less hard to refute, and must at all events be pronounced self-consistent. Mr. Mivart takes up different ground. He admits man to be an animal, but yet proclaims him to be an animal differing more widely from those nearest him in structure, such as the gorilla, than they do from the unorganised lifeless sand beneath their feet. This somewhat sensational deliverance occurs, in substance, more than once, so that it is no mere casual inadvertence. Let us look more clearly into its meaning. Let A denote inorganic matter, B the vegetable world, and C the animal kingdom. In the class C occurs a certain form, *c*, which differs more widely from the other members of the class, *a*, *b*, *d*, &c., than they do from B, or even from A. What kind of classification is this? If *c* differs thus widely from everything else contained in C, we doubt its right to be included in that class at all. Let us take a few instances:—Suppose a curvilinear figure differing more widely from other curvilinear figures than they do in turn from rectilineal figures; suppose a crystal differing more from other crystals than they do from amorphous matter; suppose an acid differing more widely from other acids than they do from bases; suppose a triad differing more widely from other triads than they do from dyads or tetrads; suppose a shade of red differing more widely from other shades of red than they do from yellows or blues; suppose a bird differing more widely from other birds than they do from mammals! Let our readers, if they can, suppose some, any, or all of this, and they will be in a position to understand and appreciate Mr. Mivart's exposition of man's rank in creation. We fear that if any of the "Agnostics" had made a statement half so peculiar it might have received a notice more outspoken than courteous.

Mr. Mivart makes no attempt to base the distinction between man and the lower animals upon points of structure,—in short, upon anything visible. He is far too profoundly versed in animal morphology to make such an attempt. Nay, in a most interesting little work, he has declared that the structure of the frog is by far more isolated and exceptional, with reference to other forms of animal life, than is that of man. Now, when an army retreats from the open country into quagmires, forests, and deserts, both enemies and neutral on-lookers regard the movement as a confession of weakness. In the very same manner, when a doctrine or a theory changes its ground and recedes, opponents know what this implies. The doctrine of abiogenesis has thus receded. Time was when the world believed that insects of highly complicated organisation could thus be produced. Now it is held, if at all, only with regard to bacteria, invisible to the naked eye. In like manner the doctrine of the "great gulf" was once maintained on points of structure, visible and tangible marks. Now all these supposed characteristics are given up, and the alleged distinction is based on matters invisible—points to be inferred or guessed at. The signification of such a retreat is immense. Mr. Mivart rests his case on a triple assertion :—

> Man has language ; brutes have none.
>
> Man has reason ; brutes have merely instinct or *quasi*-intelligence.
>
> Man has an innate perception of right and wrong ; brutes are devoid of moral life.

The three distinctions here brought forward are by no means novel. They have all been previously adopted, and have all in turn been explicitly or tacitly rejected by thinkers who still admit a difference of kind between man and beast. Mivart combines them all, doubtless in the hope that if two wrongs do not make one right, three may possibly be found adequate. We do not find that he is able to bring forward, on any of these points, any argument which may not fairly be considered as already refuted.

The claims of language as a decisive criterion have been urged by Prof. Max Müller—a high authority, doubtless, on human tongues, but, we submit, scarcely so well acquainted with the languages of brutes as to warrant him in pronouncing on the question. Popular opinion, embodied in the phrase "dumb animals," takes a similar view ; but dumb means, after all, little more than speaking a language which we cannot understand. The ancient Greek and the modern Pole both pronounced their neighbours, of different races, "tongueless," or "mute." On the other hand, Quatrefages, a believer in the "great gulf," and a most decided unbeliever in Mr. Darwin, is of opinion that language does not constitute the boundary line. The late Archbishop Whateley was, we believe, of the same opinion.

But turning from authorities, how eminent soever, let us consider that domestic animals have been found capable of understanding words addressed to them, or merely uttered in their presence, of a more complicated nature than a mere command, and where the tone and gestures of the speaker could supply no clue to the meaning of the speaker.* Now we consider it self-evident that a being absolutely devoid of language, and therefore not fitted for receiving communications from without through any such medium, could at all understand the language of man. Mr. Mivart would probably pronounce all such instances "sensational," and seek to get rid of them by the very compendious process of denial—the way in which inconvenient facts are commonly treated by men of "first principles." We hold, however, that cases of this nature are far too numerous and too well established to be thus summarily dismissed. That the words were actually understood was shown by the events, and the events are generally recorded by observers who had no theory either to defend or to overthrow.

The languages of animals may, doubtless, be poor in abstract terms; but even Prof. Max Müller admits that in human languages abstractions are expressed by words originally concrete in their meaning. Coleridge was of opinion that thought and language were not necessarily connected, and that, had the latter never originated, mankind might have been able to reason without it, and perhaps in a superior manner. The reasoning process in animals may thus be conducted without anything equivalent to words.

It must be further considered that language does establish a break much lower down in the animal kingdom. There are animals which have demonstrable organs of hearing, which possess voices, or instead are endowed with delicate instruments for communicating their meaning by signs. On the other hand, there are other animals which have neither voices nor organs for exchanging signs, nor, as far as we can observe, any auditory apparatus. Surely, then, if we are to take "language" as the test, there is a greater gap, a more complete break, between such absolutely dumb animals and those which can at all events call to each other. Surely there is a wider difference between "nothing" and "something" than between "something" and a greater and more perfect something. The difference of *kind*, according to the language criterion, does not fall between man and apes, but between the higher animals—man included—and certain of the very lowest. We strike out, therefore, at once, the first of Mr. Mivart's three points.

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* See Quarterly Journal of Science, v., 70, 71.

What, then, of reason? Is it the exclusive attribute of man? Here, again, we have on our side the suffrages of men perfectly free from the least trace of Darwinian views. Cicero ascribes to the ant—"Mens, ratio et memoria." Milton, a man familiar with metaphysical and scholastic subtleties, makes one of his angels say concerning the lower animals—

> "They also reason, not contemptibly."

The orthodox Cuvier, antagonistic as he was to Evolutionism in every guise, speaking of brutes, declares—"Leur intelligence exécute des opérations du même genre." Agassiz holds that we cannot draw any definite boundary between the faculties of a young child and those of a baby-chimpanzee.

We are told, indeed, that were animals rational they would be capable of using language which we could understand. This by no means follows. To us it seems more than merely probable that a great difference in the degree of the mental faculties on either side may be quite sufficient to account for the imperfect understanding that prevails between brutes and ourselves. Perhaps beings as much superior to us as we are to *Acari* may be at this very moment rejecting our claims to reason as flatly as Mr. Mivart rejects those of "our poor relations."

An attempt is also made to show that if working naturalists consider animals to be rational, it is because they do not know what reason is. They ought, forsooth, to study metaphysics, and then might rise to a belief in the great gulf! This suggestion reminds us of the fox, in the fable, who had lost his tail in a trap, and who promised great advantages to his companions if they likewise would submit to amputation. Of course if we allow Mr. Mivart to frame a definition of reason to suit his own objects, the result may be foreseen. We hold that "reason," like "life" or like "poison," may be much more usefully illustrated than defined. We find animals arriving at results similar in nature, though of a lower degree, than what we attain ourselves. We conclude, therefore, that they reach these results not by the aid of a totally different faculty, gratuitously assumed for the occasion, but by a lower grade of the power which we acknowledge in ourselves. Brutes can, as we see, trace effects to their causes; they can devise means to an end under circumstances which forbid recourse to the usual explanation of instinct; they can invent; can be struck with an inward suggestion, can try its feasibility, and put it into execution. They are even not altogether unable to deal with pure abstractions. Let us take a significant, though very simple, case. Suppose a man required to carry two boxes, not heavy, but too bulky to be conveniently grasped at once. A "happy thought" strikes him; he examines them, compares their sizes, finds that one can be conveniently nested within the other, and completes his task with ease. This case, simple as it is, manifestly involves reason. Nay, most of our readers will have met with men who, when they encounter some such difficulty, seem incapable of devising any expedient to escape it. What, then, must we say of the dog referred to in the following case?—"One of the most unmistakable examples of dog-reason I can call to mind is that of a Newfoundland dog sent across a stream to fetch a couple of hats, whilst his master and a friend had gone on some distance. The dog went after them, and the gentlemen saw him attempt to carry both hats, and fail, for the two were too much for him. Presently he paused in his endeavour, took a careful survey of the hats, discovered that one was larger than the other, put the small one in the larger, and took the latter in his teeth by the brim." * Or, again, suppose that a savage observes game frequenting a certain track in a forest. A suitable locality suggests to him the possibility of catching them by a stratagem. He makes an experiment to test the practicability of the scheme, and feeling satisfied on this score, puts it in successful operation. Were such a case narrated it would be at once accepted as a proof of reason in the savage, and might be made the subject of much sensational comment. Yet here is the very action performed not by "a man and a brother," but by a fox:—"On coming home from shooting I observed, at some distance, a fox jumping continually up to a trunk of a tree of a middling height, holding something in his mouth. On examination I saw it to be a branch of a considerable size. Anxious to learn the reason I laid myself quietly down. In a very short time the fox laid down the branch and sat down on the trunk, prepared for a jump. Soon after I heard the approach of a family of wild pigs, which after some time were quite near to the stump. At the moment when they passed the fox, he jumped down on one of the young pigs, and returned with it to his elevated perch, preparing himself to begin a fat breakfast, quite careless of the impotent anger of the wild sow."† With the man who can venture to refer

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* Shirley Hibberd, Clever Dogs, &c.
† Zoologist, p. 1365.

this case to instinct it is a mere waste of time to argue. Those who dispute the fact are reminded that lions have been seen in a very similar manner practising a manœuvre, and even conferring together on the subject.*

The following fact, which has never been questioned, is a clear case of a discovery made by insects, and forthwith turned to practical account:—Ants have been observed, both by Réaumur and Bonnet, to place their eggs between the outer wooden casing and the inner panes of a glass bee-hive, a situation where, without any trouble on their part, a regular and sufficient temperature exists. By so doing they are enabled to dispense with a great amount of labour in removing the eggs from one part of the nest to another, according to the weather. On this subject Messrs. Kirby and Spence remark†—"It is impossible to account for this without supposing some stray ant that had insinuated herself into this tropical crevice first to have been struck with the *thought* of what a prodigious saving of labour and anxiety would accrue to her compatriots by establishing their society here; that she had communicated her views to them, and that they had resolved upon an emigration to this newly-discovered country, whose genial climate presented advantages which no other situation could offer. Neither instinct nor any conceivable modification of instinct could have taught the ants to avail themselves of a good fortune which, but for the invention of glass hives, would never have offered itself to these insects. The conclusion seems irresistible that reason must have been their guide, inducing a departure from their ordinary habits." We may here ask—If observation and subsequent reflection can induce an animal to depart from its ordinary habits, are not those habits themselves under the direction of reason?

One case more, typical of a very important class, must be brought forward. Number, it will be conceded, is an abstract idea. A work was written but a little while ago‡ to prove the inability of animals to comprehend even the simplest numerical relations. There is, however, an instance on record of a Scotch collie, who, when assisting at the operation of sheep-washing, showed himself equal to count quite as well as many savages. There was close to the stream a small pen, capable of holding, if we remember rightly, eleven sheep at a time. The dog, without any assistance, always started off to the flock and drove up the sheep in successive lots of eleven, without ever committing an error. We are unable to see how even the most adroit sophist can explain away this case. Man, however, is very loth to yield his fancied superiority. If the actions of animals can no longer be all explained by "instinct," surely some new name can be invented! Words are very cheap, and if they signify nothing where is the harm? Accordingly we have a new set of faculties, to which the actions of brutes may be ascribed. We hear of "*quasi*-intelligence," "*quasi*-mind," and even of "*quasi*-memory." Perhaps we shall in due time be informed that when an animal is in need of food it feels "*quasi*-hunger," and that when overdriven it suffers from "*quasi*-fatigue." Is it not gratuitous and unphilosophical in the extreme thus to multiply imaginary faculties? If it can be positively proved, from facts, that a dog remembers persons, places, or events by a totally different process and on totally different principles from what we ourselves do, then it will be time to talk of "*quasi*-memory." Until such proof is furnished it is a mere insult to our common sense. More than that, it is the *reductio ad absurdum* of all systems and first principles from which such a conclusion can be drawn. We trust that our physicists and chemists may not catch this infection, and treat us to *quasi*-magnetism, *quasi*-light, and *quasi*-gravitation.

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* R. Moffat, Missionary Labours and Scenes in Southern Africa.
† Kirby and Spence, Entomology, ii., p. 416.
‡ See Quarterly Journal of Science, v., 361.

It is suggested that a book should be written on the stupidity of animals. Such a work might then be very appropriately followed up by a companion volume on the stupidity of mankind. We fear that the latter, if fairly compiled, would prove the bulkier of the two. We are told that an elephant at the Zoological Gardens, finding the end of its trunk entangled in a ring, pulled till it tore off the extremity of its own member; but we know of a man who, in pruning his orchard, deliberately and neatly sawed away the branch against which his ladder was leaning, and fell to the ground with great violence. His name was Ferdinand Hilthel, and he lived not fifteen miles from Goerlitz. Yet on the strength of such negative cases we should not be justified in pronouncing man irrational. Why then should such an inference be drawn from the occasional, or even frequent, stupidity of the lower animals?

As a proof of animal irrationality it is said that a dog has been known, in a sudden broil, to fly *at* his master. We do not in the least dispute it, for we have seen very similar blunders committed by man! We witnessed an instance where a gentleman, in the confusion consequent upon a railway-train arriving much behind its time at a crowded station, actually sought to prevent his wife from getting into the same compartment as himself, whilst all the time he was most anxious to secure a seat for her. Are we to pronounce him endowed merely with "*quasi*-intelligence"?

We may surely decide that the attempt to erect "reason" into an absolute criterion for distinguishing between man and beast is an utter failure, and that its hopelessness will be more and more recognised the more profound and the more accurate our knowledge of the animal world becomes. Vast as is the superiority of our own species over even the highest brute, the difference is not of kind, but of degree.

We cannot help pointing out as significant the assertion that if the ants could be proved to be rational they would be insects merely in form!

We pass on to the last remaining point, the moral life, and ask if here can be found the absolute distinction which, mirage-like, has fled as we have followed? One of the charges brought against the so-called "Agnostics" is that they confound virtue with pleasure. This accusation is urged in a chapter in which he seeks to show that "perceptions of right and wrong, and of our power of choice and consequent responsibility, are universally diffused amongst mankind, and constitute an absolute character separating man from all other animals." Here, as usual, dissidents are criticised sometimes singly and sometimes collectively, all being, by implication at least, held answerable for any error or oversight, real or imaginary, detected in the writings of any one, and for its assumed consequences. Now, that some modern writers may have forgotten that an action highly pleasurable to the doer is not necessarily virtuous, we shall not seek to deny. In so doing they have been probably influenced by a more or less conscious reaction against the opposite error so dominant in the Dark Ages, and at all other times of rampant ecclesiasticism—that an action was to be regarded as vicious in the exact proportion of its pleasurableness, even though no person were injured, whilst sufferings and privations by which no one was benefitted were deemed virtuous and meritorious. Of these two opposite errors the modern one is assuredly the less dangerous. But it is very curious that Mr. Mivart, well acquainted as he must be with the writings of Mr. Herbert Spencer, has not thought proper to allude to his criterion for distinguishing evil from good. To this we are therefore obliged to call attention:—"From whatever assumptions they start, all theories of morality agree that conduct whose total results, immediate and remote, are beneficial, is good conduct; whilst conduct whose total results, immediate and remote, are injurious. is bad conduct." * This passage, we submit, completely refutes the charge brought against modern philosophy—that it teaches man to indulge any desire, no matter at what cost to others, or with what future consequences to himself, Ultimate as well as immediate results are considered, and the effects—not upon the actor alone, but upon all persons whom the action can possibly influence—are fully weighed.

To Mr. Spencer's criterion it has been objected that it leaves motives out of the question. But we are not sure that the consideration of motives can be, generally speaking, anything but delusive. What may be the motives which have led to any particular line of conduct we may guess with more or less of correctness, but we can never know with certainty. Nor in many cases can motives, however accurately known, be allowed to weigh in the same scale as results, or in any manner to affect our judgment. We should surely not show any more mercy to a Thug, or to a judge of the "Holy Office," who murders men "for their soul's health" or for the pretended honour of his God, than we would to a Greek brigand or a Chinese pirate. If anything, the latter are the less dangerous criminals. Mr. Mivart dispenses with every criterion, holding man's notions of right and wrong to be intuitive: his manner of dealing with the innumerable facts that prove, on the contrary, that man has no such innate standard, is saddening. The strangest scepticism, on the one hand, is blended with a credulity no less strange on the other. The fact that no authenticated instance of remorse on the part of a savage can be adduced is left in the background. The outrages of wild men are sought to be explained away with a wonderful amount of misplaced ingenuity. We quote the following passage:—"Thus the most revolting act that can well be cited, that of the deliberate murder of aged parents, monstrous as the act in itself is, may really be one of filial piety, if, as is asserted, the savage perpetrators do it at the wish of such parents themselves, and from a conviction that thereby they not only save them from suffering in this world, but also confer upon them prolonged happiness in the next." It is a known fact that the murder of aged relatives is often effected in such a way that the victims would never solicit it as a favour. Sometimes they are disposed of not by being promptly slain, but by being simply abandoned to die of

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* Essay on Education, p. 114.

hunger and thirst, or to be devoured by wild beasts, as the chance may be. Can we believe that any person would wish to be thus "saved from suffering in this world"? But we need not confine our attention to the lowest savages. The ancient Danes, as is very well known, had a custom of tossing young children upon the points of their spears. Was this act, also, a species of disguised kindness, intended merely for the good of the victims? Surely an innate moral sense which can allow such actions as we have here mentioned, not to speak of what might further be brought forward, must be of no practical value. It is idle to say that savages do not approve of murder, robbery, and outrage, because they become angry if themselves or their tribe are the sufferers. The lower animals do just the same: rob a wild beast of his prey, or of his young, and your life is in peril. Shoot a peccary, and the whole tribe rushes upon you.

Mr. Mivart quotes as an example of "Savage refinement" the following passage from Sir John Lubbock:—

"Among the Greenlanders, should a seal escape with a hunter's javelin in it, and be killed by another man afterwards, it belongs to the former. But if the seal is struck with the harpoon and bladder and the string breaks, the hunter loses his right. If a man finds a seal dead with a harpoon in it, he keeps the seal but returns the harpoon. Any man who finds a piece of drift-wood can appropriate it by placing a stone on it as a sign that some one has taken possession of it. No other Greenlander will then touch it."*

This is very interesting; but we can give from our own observation a case somewhat similar amongst animals certainly not ranking high in the scale of intelligence. We kept formerly a number of vipers and other snakes in a pit something like a melon frame. If a live mouse was dropped into the pit there was a general scramble, and all the venomous inmates were seen snapping at the warm-blooded intruder. But as soon as one had planted a fatal bite all the others withdrew into their crevices, or coiled themselves up to sleep, leaving the conqueror to the quiet enjoyment of his meal. This we witnessed repeatedly, and can bear witness that it was not the largest or strongest snakes alone whose rights to their prey were thus left undisputed.

But even if it were shown that all existing tribes on the earth had some notions of morality the question is still open. Mr. Mivart assumes that no tribes ruder and lower than any now dwelling upon our globe have flourished in primeval days, and have been swept away. Yet that such must have once existed will appear highly probable if we reflect on the process of extermination still going on. Is it not likely that in more barbarous days before modern philanthropy had arisen, and before the Aborigines Protection Society had been organised, wars of extirpation, always directed against the races least raised above the brute level, would be more frequent and more destructive? The legends of all ancient nations point in the same direction, telling us of half-human monsters whom their forefathers extirpated or drove out.

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* Origin of Civilisation, p. 305.

But this is not all; before the presence of a moral sense, of a feeling of right and wrong, whether innate or acquired, can be brought forward as "an absolute characteristic separating man from all other animals," it must be shown that no other animal possesses such moral sense. And here Mr. Mivart has nothing save baseless, wanton assumption to array against solid facts. He may, if it so please him, *assert* that brutes are void of all traces of conscience, but unless he could enter into their minds and be aware of their feelings, such assertion is unwarrantable. Suppose a mineralogist were to hold up before us two minerals, A and B, very similar in all their outward properties, and should inform us that the distinction between them consisted in the fact that cobalt was always present in A, and was as uniformly absent in B. We should naturally ask if he had analysed both and could give a full account of all their constituents? What should we think were he to reply:—"I certainly have analysed A, and have found the presence of cobalt. B I have not been able to analyse, but on *a priori* grounds I am satisfied that no cobalt can there be present." Should we not feel inclined to send him back to take some quite other "lessons from nature?"

As an instance of the very different conclusions which other minds have drawn from a close and prolonged observation of animal life, we may take the following passage from the writings of the late Agassiz. Pronouncing the range of passion in animals as extensive as in man, he continues:—"I am at a loss to trace a difference of kind between them. The gradation of moral faculties between the higher animals and man is so imperceptible that to deny to the first a certain sense of consciousness and responsibility would be an exaggeration." These are the words, it must be remembered, of an original observer of unquestioned ability and untiring industry, who, moreover, devoted his attention far more closely and exclusively to biology than Mr. Mivart has apparently done. Agassiz, further, was no "Agnostic," no Darwinist, no believer in Evolution, but a champion of the doctrine of individual creation. No one can accuse him of seeking to under-estimate the difference between man and other animals from any sinister motive.

The Rev. J. G. Wood, in his recent interesting work "Man and Beast,"—without, as far as we can perceive, accepting Darwinism, or even Evolution, and certainly without seeking to demonstrate our kinship with apes,—arrives at conclusions closely resembling those of Professor Agassiz, and even produces no contemptible evidence in favour of animal immortality. The like has been done by Bishop Butler. Nor can it be denied that some at least of the strongest arguments advanced in favour of man's immortality tell in favour of a hereafter in store for lower animals. If the life of man is a drama, of which the fifth act, with its compensations and retributions, is reserved for another stage, surely the same should hold good with brutes, among whom also there prevail those differences of destiny which have perplexed man.

There are on record fully authenticated instances of animals feeling ashamed of actions they have committed. We may refer to the case observed and described by Mr. G. J. Romanes.* This case, which we think no one will attempt to ignore as the exaggeration or the mistake of an incompetent observer, is very significant. To escape ridicule X is tempted to tell (or not) a falsehood. Detected in this the said X feels much more distressed and ashamed than when merely ridiculed for his blundering. Now if for X we read John Nupkins, all will admit that John Nupkins knew that falsehood was wrong, and will call his subsequent distress the action of a guilty conscience. But if, instead of John Nupkins, X happens—as in this case—to stand for a terrier, where is our right to put any different interpretation on the same set of facts?

Among certain birds—*e.g.*, rooks—careful observers have detected distinct traces of criminal law. Thievish birds, who persevere in stealing sticks from the nests of their fellow-citizens, have been seen banished from the community, severely chastised, and even killed, by a general assemblage. "But law necessarily pre-supposes the notions of right and wrong, and could never, therefore, have arisen among beings incapable of drawing this distinction."

We shall add one more case to prove in the lower animals the existence of free will, the power of overcoming natural instincts and temptations, in order to secure a supposed benefit in the sequel ;—" A fine terrier, in the possession of a surgeon at Whitehaven, about three weeks ago exhibited its sagacity in a rather amusing manner, It came into the kitchen and began plucking the servant by the gown, and, in spite of repeated rebuffs, it perseveringly continued in its purpose. The mistress of the house, hearing the noise, came down to enquire the cause, when the animal treated her in a similar manner. Being struck with the concern evinced by the creature, she quietly followed it upstairs into a bed-room, whither it led her; there it commenced barking, looking under the bed, and then up in her face. Upon examination, a cat was discovered there quietly demolishing a beef-steak, which it had feloniously obtained. The most singular feature in the whole case is that the cat had been introduced into the house only a short time before, and that bitter enmity prevailed between her and her canine companion."* This is a capital case. "Instinct" would undeniably have led the terrier to attack the cat and attempt to deprive her of her booty, the rather as the two animals were on unfriendly terms. But we find this natural impulse here completely restrained for the attainment of a certain definite end. The terrier lays an information against his enemy. Why should he, unless he entertained the notion that theft was wrong? He evidently concluded that his enemy, if detected in such an act, would probably suffer severe punishment. The incident is of the greater value as it prove that brutes are capable not merely of planning means to effect an object quite unconnected with the preservation of the individual or of the species, but of exercising self-control; that, in short, they do not always blindly and necessarily follow their physical appetites, but can, like man, forego present indulgence for what appears to them a greater good hereafter.

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* See Quarterly Journal of Science, v., 425., and p. 153 of present number.

A strange attempt is made to show that animals are altogether unconscious; that though they feel pain, they are not aware of so doing. They are represented as being, therefore, naturally and permanently in the state into which man may be artificially and temporarily thrown by means of anæsthetics. If this be, then cruelty to animals is an impossibility, and the stipulation that in vivisection anæsthetics are to be employed is farcical in the extreme. But what of the evidence for this assumption? We know that man can be conscious of suffering, and also, under certain circumstances, unconscious. We are not without grounds for supposing that the lower animals feel less acutely than he does.* But how are we to be certain that as a class they are always unconscious of pain? Who has looked into the mind of a tortured horse or dog, and satisfied himself that it was unconscious of its misery, and therefore not miserable? And how is the favoured sage who has done this wondrous thing to satisfy us that he has observed and interpreted aright? Mr. Mivart tells us that a wasp deftly snipped in two with a pair of scissors, whilst sipping honey or syrup, will continue its banquet. We have seen an impaled dragon-fly greedily devour a blue-bottle presented to his jaws. The absence of struggle and disturbance is here taken as a proof that the animal does not feel. But if so, when we find a wounded animal writhing and screaming, may we not infer that it both feels and knows that it feels?

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* Zoologist," p. 2131.

What of mental distress, sorrow, as distinct from bodily pain? We know that this condition produces in brutes the very same results as in man. Dogs have been known to pine away and die for the loss of their masters. Female apes sometimes, "sensational" as it may seem, do not always recover from the effects of losing their infants. Birds have often drooped and died on losing their mates. A horse sometimes falls out of condition on parting with its yoke-fellow. Yet we are to believe that they are all the while unconscious of the distress they suffer! In short, for the notion of the total unconsciousness of animals, we can find no valid evidence, but much against it, and must therefore dismiss it to limbo as one of the many far-fetched and hopeless attempts to defend the "great gulf."

We do not consider it legitimate to denounce any scientific hypothesis because some persons may find in it countenance for moral or social errors. But we cannot help pointing out that, had this unconsciousness of animals been advanced as a cardinal point of Darwinism, great would have been the outcry raised against the demoralising tendencies of modern science. Here, however, science pleads on the side of mercy, whilst Mediævalism replies to every attempt to lighten the sufferings of domestic animals: "No es Cristiano!"

The whole work, despite the unquestionable ability which it evinces, must be pronounced disappointing—worthy, perhaps, of a Joseph de Maistre, but utterly unworthy of a

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* It is probable also that the lower races, or species—if we may venture to use the word—of mankind have less feeling than the higher.

Mivart. If such are genuine "Lessons from Nature" the venerable dame keeps a most inefficient school, and the sooner it is closed the better. But in this case the pure white light has passed through such a powerfully distorting medium that its true nature can scarcely be recognised. Metaphysics may be regarded as a disease which thinkers are liable to contract at some part of their career, just as children take the measles, or rather the scarlet fever. Mr. Mivart's is a very bad case; but for his own sake, and still more for that of Science, we wish him a full and a speedy recovery. An intellect like his is too valuable to be lost.

V. THE MECHANICAL ACTION OF LIGHT.*

By W. Crookes, F.R.S.

To generate motion has been found a characteristic common, with one exception, to all the phases of Physical Force. We hold the bulb of a thermometer in our hands, and the mercury expands in bulk, and, rising along the scale, indicates the increase of heat it has received. We heat water, and it is converted into steam, and moves our machinery, our carriages, and our ironclads. We bring a loadstone near a number of iron filings, and they move towards it, arranging themselves in peculiar and intricate lines; or we bring a piece of iron near a magnetic needle, and we find it turned away from its ordinary position. We rub a piece of glass with silk, thus throwing it into a state of electrical excitement, and we find that bits of paper or thread fly towards it, and are, in a few moments, repelled again. If we remove the supports from a mass of matter it falls, the influence of gravitation being here most plainly expressed in motion, as shown in clocks and watermills. If we fix pieces of paper upon a stretched string, and then sound a musical note near it, we find certain of the papers projected from their places. Latterly the so-called "sensitive flames," which are violently agitated by certain musical notes, have become well known as instances of the conversion of sound into motion. How readily chemical force undergoes the same transformation is manifested in such catastrophes as those of Bremerhaven, in the recent deplorable coal-mine explosions, and indeed in every discharge of a gun.

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* A Lecture delivered at the Royal Institution, on Friday evening, February 11th, 1876.

But light, in some respects the highest of the powers of Nature, has not been hitherto found capable of direct conversion into motion, and such an exception cannot but be regarded as a singular anomaly.

This anomaly the researches which I am about to bring before you have now removed; and, like the other forms of force, Light is found to be capable of direct conversion into motion, and of being—like heat, electricity, magnetism, sound, gravitation, and chemical action—most delicately and accurately measured by the amount of motion thus produced.

My research arose from the study of an anomaly.

It is well known to scientific men that bodies appear to weigh less when they are hot than when they are cold; the explanation given being that the ascending currents of hot air buoy up the body, so to speak. Wishing to get rid of this and other interfering actions of the air during a research on the atomic weight of thallium, I had a balance constructed in which I could weigh in a vacuum. I still, indeed, found my apparatus less heavy when hot than when cold. The obvious explanations were evidently not the true ones: *obvious* explanations seldom are true ones, for simplicity is not a characteristic of Nature.

An unknown disturbing cause was interfering, and the endeavour to find the clue to the apparent anomaly has led to the discovery of the mechanical action of light.

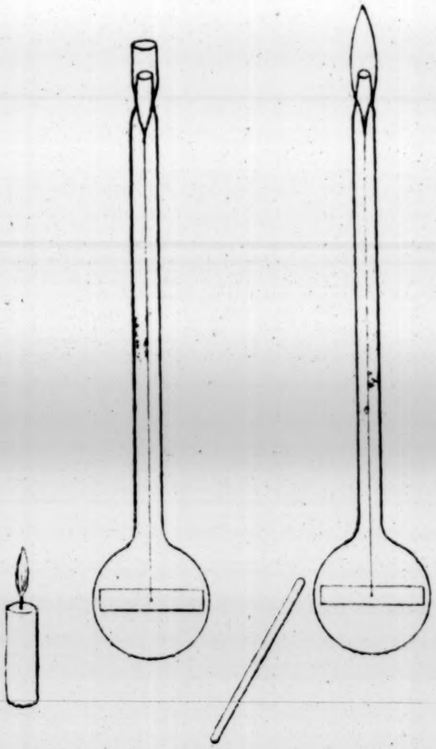
I was long troubled by the apparent lawlessness of the actions I obtained. By gradually increasing the delicacy of my apparatus I could easily get certain results of motion when hot bodies were brought near them, but sometimes it was one of attraction, at others of repulsion, whilst occasionally no movement whatever was produced.

I will try to reproduce these phenomena in this apparatus (Fig. 1). Here are two glass bulbs, each containing a bar of pith about 3 inches long and ½ an inch thick, suspended horizontally by a long fibre of cocoon silk. I bring a hot glass rod, or a candle, towards one of them, and you see that the pith is gradually attracted, following the candle as I move it round the bulb. That seems a very definite fact; but look at the action in the other bulb. I bring the candle, or a hot glass rod, near the other bar of pith, and it is strongly *repelled* by it—much more strongly than it was attracted in the first instance.

Here, again, is a third fact. I bring a piece of ice near the pith bar which has just been repelled by the hot rod, and it is attracted, and follows the rod round as a magnetic needle follows a piece of iron.

The repulsion by radiation is the key-note of these researches. The movement of a small bar of pith is not very distinct, except to those near, and I wish to make this repulsion evident to all. I have therefore arranged a piece of apparatus by which it can be seen by all present. I will, by means of the electric light, project an image of a pendulum suspended *in vacuo* on the screen. You see that the approach of a candle gives the bob a veritable push, and, by alternately obscuring and uncovering the light, I can make the pendulum beat time to my movements.

FIG. 1.

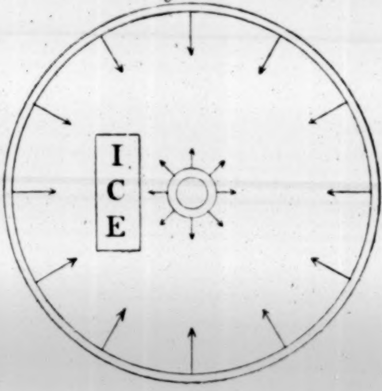


What then is the cause of the contradictory action in these two bulbs—attraction in one, and repulsion in the other? It can be explained in a few words. Attraction takes place when air is present, and repulsion when air is absent.

Neutrality, or no movement, is produced when the vacuum is insufficient. A minute trace of air in the apparatus interferes most materially with the repulsion, and for a long time I was unaware of the powerful action produced by radiation in a "perfect" vacuum.

It is not at first sight obvious how ice or a cold body can produce the opposite effect to heat. The law of exchanges, however, explains this perfectly. The pith bar and the whole of the surrounding bodies are incessantly exchanging heat-rays; and under ordinary circumstances the income and expenditure of heat are in equilibrium. Let me draw your attention to the diagram (Fig. 2) illustrating what takes place when I bring a piece of ice near the apparatus. The centre circle represents my piece of pith; the arrows show the influx and efflux of heat. A piece of ice brought near cuts off the influx of heat from one side, and therefore allows an excess of heat to fall on the pith from the opposite side. Attraction by a cold body is therefore seen to be only repulsion by the radiation from the opposite side of the room.

Fig 2.



The later developments of this research have demanded the utmost refinement of apparatus. Everything has to be conducted in glass vessels, and these must be blown together till they make one piece, for none but fused joints are admissible. In an investigation depending for its successful prosecution on manipulative dexterity, I have been fortunate in having the assistance of my friend Mr. Charles Gimingham. All the apparatus you see before you are the fruits of his skilful manipulation, and I now want to draw your attention to what I think is a masterpiece of glass-working —the pump which enables me so readily to produce a vacuum unattainable by ordinary means.

The pump here at work is a modification of the Sprengel pump, but it contains two or three valuable improvements. I cannot attempt to describe the whole of the arrangements, but I will rapidly run over them as illuminated by the electric light. It has a triple fall tube in which the mercury is carried down, thus exhausting with threefold rapidity; it has Dr. McLeod's beautiful arrangement for measuring the residual gas; it has guages in all directions, and a small radiometer attached to it to tell the amount of exhaustion that I get in any experiments; it has a contrivance for admitting oil of vitriol into the tubes without interfering with the progress of the exhaustion, and it is provided with a whole series of most ingenious vacuum-taps devised by Mr. Gimingham. The exhaustion produced in this pump is such that a current of electricity from an induction-coil will not pass across the vacuum. This pump is now exhausting a torsion-balance, which will be described presently. Another pump, of a similar kind but less complicated, is exhausting an apparatus which has enabled me to pass from the mere exhibition of the phenomena to the obtaining of quantitative measurements.

A certain amount of force is exerted when a ray of light or heat falls on the suspended pith, and I wished to ascertain—

First. What were the actual rays—invisible heat, luminous, or ultra violet—which caused this action?

Secondly. What influence had the colour of the surface on the action?

Thirdly. Was the amount of action in direct proportion to the amount of radiation?

Fourthly. What was the amount of force exerted by radiation?

I required an apparatus which would be easily moved by the impact of light on it, but which would readily return to zero, so that measurements might be obtained of the force exerted when different amounts of light acted on it. At first I made an apparatus on the principle of Zöllner's horizontal pendulum. For a reason that will be explained presently I am unable to show you the apparatus at work, but the principle of it is shown in the diagram (Fig. 3). The pendulum represented by this horizontal line has a weight at the end. It is supported on two fibres of glass, one stretched upwards and the other stretched downward, both firmly fastened at the ends, and also attached to the horizontal rod (as shown in the figure) at points near together, but not quite opposite to one another.

It is evident that if there is a certain amount of pull upon each of these fibres, and that the pull can be so adjusted as to counteract the weight at the end and keep it horizontal, the nearer the beam approaches the horizontal line the slower its rate of oscillation. If I relax the tension, by throwing the horizontal beam downwards, I get a more rapid oscillation sideways. If I turn the levelling screw so as to raise the beam and weight, the nearer it approaches the horizontal position the slower the oscillation becomes, and the more delicate is the instrument. Here is the actual apparatus that I tried to work with. The weight at the end is a piece of pith; in the centre is a glass mirror, on which to throw a ray of light, so as to enable me to see the movements by a luminous index. The instrument, enclosed in glass and exhausted of air, was mounted on a stand with levelling screws, and with it I tried the action of a ray of light falling on the pith. I found that I could get any amount of sensitiveness that I liked; but it was not only sensitive to the impact of a ray of light, it was immeasurably more so to a change of horizontality. It was in fact too delicate for me to work with. The slightest elevation of one end of the instrument altered the sensitiveness, or the position of the zero point, to such a degree that it was impossible to try any experiments with it in such a place as London. A person stepping from one room to another altered the position of the centre of gravity of the house. If I walked from one side of my own laboratory to the other, I tilted the house over sufficiently to upset the equilibrium of the apparatus. Children playing in the street disturbed it. Prof. Rood, who has worked with an apparatus of this kind in America, finds that an elevation of its side equal to 1-36,000,000th part of an inch is sufficient to be shown on the instrument. It was therefore out of the question to use an instrument of this construction, so I tried another form (shown in Fig. 4), in which a fine glass beam, having discs of pith at each end, is suspended horizontally by a fine glass fibre, the whole

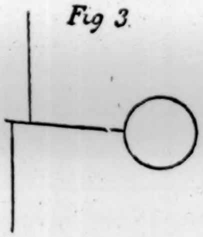


Fig 3.

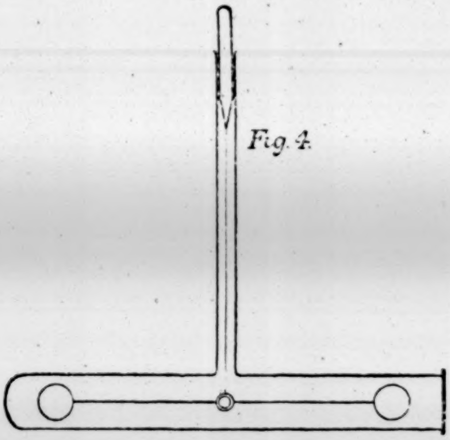


Fig. 4.

being sealed up in glass and perfectly exhausted. To the centre of oscillation a glass mirror is attached.

Now a glass fibre has the property of always coming back to zero when it is twisted out of its position. It is almost, if not quite, a perfectly elastic body. I will show this by a simple experiment. This is a long glass fibre hanging vertically, and having a horizontal bar suspended on it. I hold the bar, and turn it half round; it swings backwards and forwards for a few times, but it quickly comes back to its original position. However much twist, however much torsion, may be put on this, it always returns ultimately to the same position. I have twisted glass fibres round and kept them in a permanent state of twist more than a hundred complete revolutions, and they always came back accurately to zero. The principle of an instrument that I shall describe farther on depends entirely on this property of glass.

Instead of using silk to suspend the torsion beam with, I employ a fibre of glass, drawn out very fine before the blowpipe. A thread of glass of less than the thousandth of an inch in thickness is wonderfully strong, of great stiffness, and of perfect elasticity, so that however much it is twisted round short of the breaking point, it untwists itself perfectly when liberated. The advantage of using glass fibres for suspending my beam is, therefore, that it always returns accurately to zero after having tried an experiment, whilst I can get any desired amount of sensitiveness by drawing out the glass fibre sufficiently fine.

Here, then, is the torsion apparatus sealed on to a Sprengel pump. You will easily understand the construction by reference to the diagram (Fig. 4). It consists of a horizontal beam suspended by a glass fibre, and having discs of pith at each end coated with lamp-black. The whole is enclosed in a glass case, made of tubes blown together, and by means of the pump the air is entirely removed. In the centre of the horizontal beam is a silvered mirror, and a ray from the electric light is reflected from it on to a scale in front, where it is visible as a small circular spot of light. It is evident that an angular movement of the torsion beam will cause the spot of light to move to the right or to the left along the scale. I will first show you the wonderful sensitiveness of the apparatus. I simply place my finger near the pith disc at one end, and the warmth is quite sufficient to drive the spot of light several inches along the scale. It has now returned to zero, and I place a candle near it. The spot of light flies off the scale. I now bring the candle near it alternately from one side to the other, and you see how perfectly it obeys the force of the candle. I think the movement is almost better seen without the screen than with it. The fog, which has been so great a detriment to every one else, is rather in my favour, for it shows the luminous index like a solid bar of light swaying to and fro across the room. The warmth of my finger, or the radiation from a candle, is therefore seen to drive the pith disc away. Here is a lump of ice, and on bringing it near one of the discs the luminous index promptly shows a movement of apparent attraction.

With this apparatus I have tried many experiments, and amongst others I endeavoured to answer the question "Is it light, or is it heat, that produces the movement?"—for that is a question that is asked me by almost everyone; and a good many appear to think that if the motion can be explained by an action of heat, all the novelty and the importance of the discovery vanish. Now this question of light or heat is one I cannot answer, and I think that when I have explained the reason you will agree with me that it is unanswerable. There is no physical difference between light and heat. Here is a diagram of the visible spectrum (Fig. 5). The spectrum, as scientific men understand it, extends from an indefinite distance beyond the red to an indefinite distance beyond the violet. We do not know how far it would extend one way or the other if no absorbing media were present; but, by what we may call a physiological accident, the human eye is sensitive to a portion of the spectrum situated between the line A in the red to about the line H in the violet. But this is not a physical difference between the luminous and non-luminous parts of the spectrum; it is only a physiological difference. Now, the part at the red end of the spectrum possesses, in the greatest degree, the property of causing

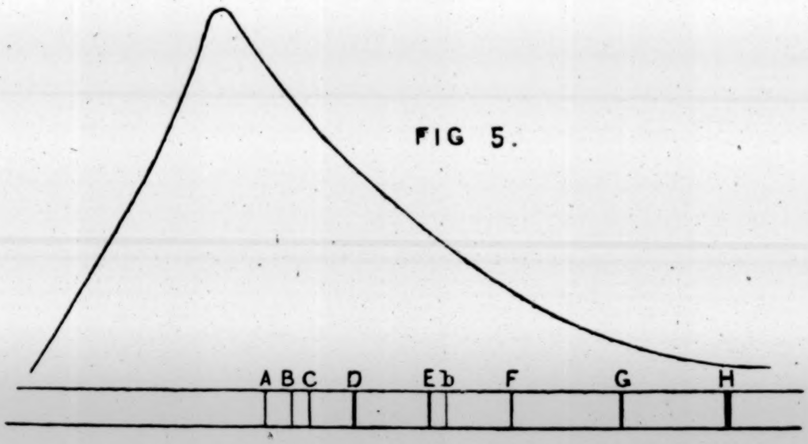


FIG 5.

the sensation of warmth, and of dilating the mercury in a thermometer, and of doing other things which are conveniently classed among the effects of *heat;* the centre part affects the eye, and is therefore called *light;* whilst the part at the other end of the spectrum has the greatest energy in producing *chemical action.* But it must not be forgotten that any ray of the spectrum, from whatever part it is selected, will produce all these physical actions in more or less degree. A ray here, at the letter C for instance in the orange, if concentrated on the bulb of a thermometer, will cause the mercury to dilate, and thus show the presence of *heat;* if concentrated on my hand I feel *warmth;* if I throw it on the face of a thermo-pile it will produce a current of *electricity;* if I throw it upon a sensitive photographic plate it will produce *chemical action;* and if I throw it upon the instrument I have just described it will produce *motion.* What, then, am I to call that ray? Is it light, heat, electricity, chemical action, or motion? It is neither. All these actions are inseparable attributes of the ray of that particular wave-length, and are not evidences of separate identities. I can no more split that ray up into five or six different rays, each having different properties, than I can split up the element iron, for instance, into other elements, one possessing the specific gravity of iron, another its magnetic properties, a third its chemical properties, a fourth its conducting power for heat, and so on. A ray of light of a definite refrangibility is one and indivisible, just as an element is, and these different properties of the ray are mere functions of that refrangibility, and inseparable from it. Therefore when I tell you that a ray in the ultra red pushes the instrument with a force of 100, and a ray in the most luminous part has a dynamic value of about half that, it must be understood that the latter action is not due to heat-rays which accompany the luminous rays, but that the action is one purely due to the wave-length and the refrangibility of the ray employed. You now understand why it is that I cannot give a definite answer to the question—"Is it heat or is it light that produces these movements?" There is no physical difference between heat and light, so, to avoid confusion, I call the total bundle of rays which come from a candle or the sun, *radiation.*

I found, by throwing the pure rays of the spectrum one after the other upon this apparatus, that I could obtain a very definite answer to my first question—"What are the actual rays which cause this action?"

The apparatus was fitted up in a room specially devoted to it, and was protected on all sides, except where the rays of light had to pass, with cotton-wool and large bottles of water. A heliostat reflected a beam of sunlight in a constant direction, and it was received on an appropriate arrangement of slit, lenses, prisms, &c., for projecting a pure spectrum. Results were obtained in the months of July, August, and September; and they are given in the figure (Fig. 5) graphically as a curve, the maximum being in the ultra-red and the minimum in the ultra-violet. Taking the maximum at 100, the following are the mechanical values of the different colours of the spectrum:—

Ultra-red	100
Extreme red	85
Red	73
Orange	66
Yellow	57
Green	41
Blue	22
Indigo	8½
Violet	6
Ultra-violet	5

A comparison of these figures is a sufficient proof that the mechanical action of radiation is as much a function of the luminous rays as it is of the dark heat-rays.

The second question, namely, "What influence has the colour of the surface on the action?" has also been solved by this apparatus.

In order to obtain comparative results between discs of pith coated with lamp-black and with other substances, another torsion apparatus was constructed, in which six discs *in vacuo* could be exposed one after the other to a standard light. One disc always being lamp-blacked pith, the other discs could be changed so as to get comparisons of action. Calling the action of radiation from a candle on the lamp-blacked disc 100, the following are the proportions obtained:—

Lamp-blacked pith	100
Iodide of palladium	87·3
Precipitated silver	56
Amorphous phosphorus	40
Sulphate of baryta	37
Milk of sulphur	31
Red oxide of iron	28
Scarlet iodide of mercury and copper	22
Lamp-blacked silver	18
White pith	18
Carbonate of lead	13
Rock-salt	6·5
Glass	6·5

This table gives important information on many points: one more especially—the action of radiation on lamp-

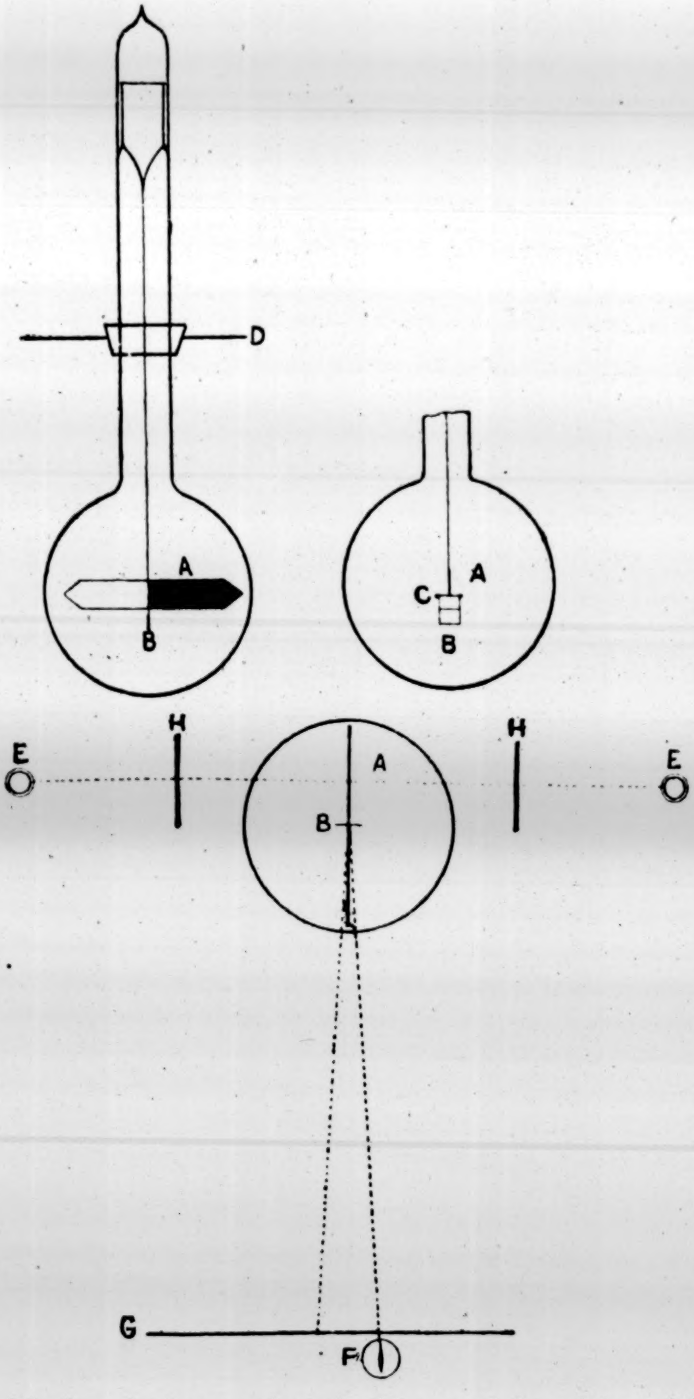


FIG. 6.

blacked pith is $5\frac{1}{2}$ times what it is on plain pith. A bar like those used in my first experiment, having one-half black and one-half white, exposed to a broad beam of radiation, will be pushed with 5½ times more strength on the black than on the white half, and if freely suspended will set at an angle greater or less according to the intensity of the radiation falling on it.

This suggests the employment of such a bar as a photometer, and I have accordingly made an instrument on this principle: its construction is shown in the diagram (Fig. 6). It consists of a flat bar of pith, A, half black and half white, suspended horizontally in a bulb by means of a long silk fibre. A reflecting mirror, B, and small magnet, C, are fastened to the pith, and a controlling magnet, D, is fastened outside so that it can slide up and down the tube, and thus increase or diminish sensitiveness. The whole is completely exhausted and then enclosed in a box lined with black velvet, with apertures for the rays of light to pass in and out. A ray of light from a lamp, F, reflected from the mirror, B, to a graduated scale, G, shows the movements of the pith bar.

The instrument fitted up for a photometric experiment is in front of me on the table. A beam from the electric light falls on the little mirror, and is thence reflected back to the screen, where it forms a spot of light, the displacement of which to the right or the left shows the movement of the pith bar. One end of the bar is blacked on each side, the other end being left plain. I have two candles, E E, each 12 inches off the pith bar, one on each side of it. When I remove the screens, H H, the candle on one side will give the pith a push in one direction, and the candle on the other side will give the pith a push in the opposite direction, and as they are the same distance off they will neutralise each other, and the spot of light will not move. I now take the two screens away: each candle is pushing the pith equally in opposite directions, and the luminous index remains at zero. When, however, I cut one candle off, the candle on the opposite side exerts its full influence, and the index flies to one end of the scale. I cut the other one off and obscure the first, and the spot of light flies to the other side. I obscure them both, and the index comes quickly to zero. I remove the screens simultaneously, and the index does not move.

I will retain one candle 12 inches off, and put two candles on the other side 17 inches off. On removing the screens you see the index does not move from zero. Now the square of 12 is 144, and the square of 17 is 289. Twice 144 is 288. The light of these candles, therefore, is as 288 to 289. They therefore balance each other as nearly as possible. Similarly I can balance a gas-light against a candle. I have a small gas-burner here, which I place 28 inches off on one side, and you see it balances the candle 12 inches off. These experiments show how conveniently and accurately this instrument can be used as a photometer. By balancing a standard candle on one side against any source of light on the other, the value of the latter in terms of a candle is readily shown ; thus in the last experiment the standard candle 12 inches off is balanced by a gas-flame 28 inches off. The lights are therefore in the proportion of 12^2 to 28^2, or as 1 to 5·4. The gas-burner is therefore equal to about 5½ candles.

In practical work on photometry it is often required to ascertain the value of gas. Gas is spoken of commercially as of so many candle-power. There is a certain "standard" candle which is supposed to be made invariable by Act of Parliament. I have worked a great deal with these standard candles, and I find them to be among the most variable things in the world. They never burn with the same luminosity from one hour to the other, and no two candles are alike. I can now, however, easily get over this difficulty. I place a "standard" candle at such a distance from the apparatus that it gives a deflection of 100 degrees on the scale. If it is poorer than the standard, I bring it nearer; if better, I put it farther off. Indeed any candle may be taken ; and if it be placed at such a distance from the apparatus that it will give a uniform deflection, say of 100 divisions, the standard can be reproduced at any subsequent time ; and the burning of the candle may be tested during the photometric experiments by taking the deflection it causes from time to time, and altering its distance, if needed, to keep the deflection at 100 divisions. The gas-light to be tested is placed at such a distance on the opposite side of the pith bar that it exactly balances the candle. Then, by squaring the distances, I get the exact proportion between the gas and the candle.

Before this instrument can be used as a photometer or light measurer, means must be taken to cut off from it all those rays coming from the candle or gas which are not actually luminous. A reference to the spectrum diagram (Fig. 5) will show that at each end of the coloured rays, there is a large space inactive, as far as the eye is concerned, but active in respect to the production of motion—strongly so at the red end, less strong at the violet end. Before the instrument can be used to measure luminosity, these rays must be cut off. We buy gas for the light that it gives, not for the heat it evolves on burning, and it would therefore never do to measure the heat and pay for it as light.

It has been found that a clear plate of alum, whilst letting all the light through, is almost, if not quite, opaque to the heating rays below the red. A solution of alum in water is almost as effective as a crystal of alum ; if, therefore, I place in front of the instrument glass cells containing an aqueous solution of alum, the dark heat rays are filtered off.

But the ultra-violet rays still pass through, and to cut these off I dissolve in the alum solution a quantity of sulphate of quinine. This body has the property of cutting off the ultra-violet rays from a point between the lines G and H. A combination of alum and suphate of quinine, therefore, limits the action to those rays which affect the human eye, and the instrument, such as you see it before you, becomes a true photometer.

This instrument, when its sensitiveness is not deadened by the powerful control magnet I am obliged to keep near it for these experiments, is wonderfully sensible to light. In my own laboratory a candle 36 feet off produces a decided movement, and the motion of the index increases inversely with the square of the distance, thus answering the third question—" Is the amount of action in direct proportion to the amount of radiation ?"

The experimental observations and the numbers which are required by the theoretical diminution of light with the square of the distance, are sufficiently close, as the following figures show :—

Candle	6 feet off	gives a deflection of	218·0°
,,	12	,,	,, 54·0°
,,	18	,,	,, 24·5°
,,	24	,,	,, 13·0°
,,	10	,,	,, 77·0°
,,	20	,,	,, 19·0°
,,	30	,,	,, 8·5°

The effect of two candles side by side is practically double, and of three candles three times that of one candle.

In the instrument just described the candle acts on a pith bar, one end of which is blacked on each side. But suppose I black the bar on alternate halves and place a light near it sufficiently strong to drive the bar half round. The light will now have presented to it another black surface in the same position as the first, and the bar will be again driven in the same direction half round. This action will be again repeated, the differential action of the light on the black and white surfaces keeps the bar moving, and the result will be rotation.

Here is such a pith bar, blacked on alternate sides, and suspended in an exhausted glass bulb (Fig. 7). I project its

FIG. 7.

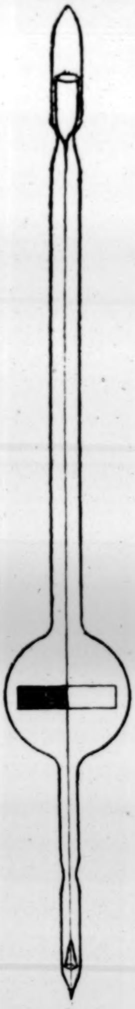
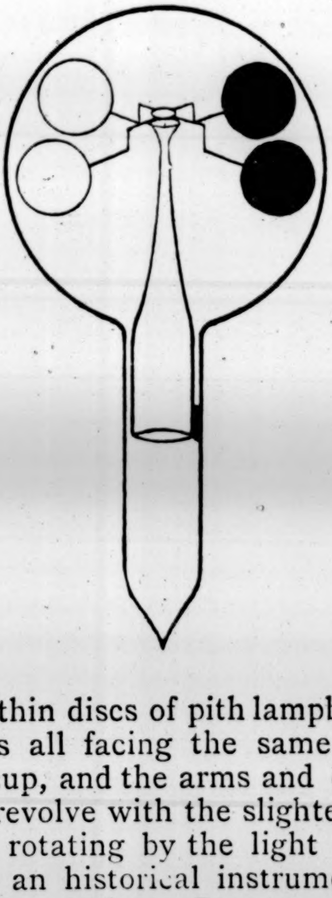


image on the screen, and the strong light which shines on it sets it rotating with considerable velocity. Now it is slackening speed, and now it has stopped altogether. The bar is supported on a fibre of silk, which has twisted round till the rotation is stopped by the accumulated torsion. I put a water screen between the bar and the electric light to cut off some of the active rays, and the silk untwists, turning the bar in the opposite direction. I now remove the water, and the bar revolves rapidly as at first.

From suspending the pith on a silk fibre to balancing it on a point the transition is slight; the interfering action of torsion is thereby removed, and the instrument rotates continuously under the influence of radiation. Many of these little pieces of apparatus, to which I have given the name of radiometers, are on the table, revolving with more or less speed. The diagram (Fig. 8) shows their construction, which is very simple. They are formed of four arms of very fine glass, supported in the centre by a needle-point, and having at the extremities thin discs of pith lampblacked on one side, the black surfaces all facing the same way. The needle stands in a glass cup, and the arms and discs are delicately balanced so as to revolve with the slightest impetus.

FIG. 8.



Here are some rotating by the light of a candle. This one is now rather an historical instrument, being the first one in which I saw rotation. It goes very slowly in comparison with the others, but it is not bad for the first instrument of the sort that was ever made.

I will now, by means of a vertical lantern, throw on the screen the projection of one of these instruments, so as to show the movement rather better than you could see it on the table. The electric light falling vertically downwards on it, and much of the power being cut off by water and alum screens, the rotation is slow. I bring a candle near and the speed increases. I now lift the radiometer up, and place it full in the electric light, projecting its image direct on the screen, and it goes so rapidly that if I had not cut out the four pieces of pith of different shapes you would have been unable to follow the movement.

The speed with which a sensitive radiometer will revolve in the sun is almost incredible; and the electric light such as I have it in this lantern cannot be far short of full sunshine. Here is the most sensitive instrument I have yet made, and I project its image on the screen, letting the full blaze of the electric light shine upon it. Nothing is seen but an undefined nebulous ring, which becomes at times almost invisible. The number of revolutions per second cannot be counted, but they must be several hundreds, for one candle has made it spin round forty times a second.

I have called the instrument the radiometer because it will enable me to measure the intensity of radiation falling on it by counting the revolutions in a given time; the law being that the rapidity of revolution is inversely as the square of the distance between the light and the instrument.

When exposed to different numbers of candles at the same distance off, the speed of revolution in a given time is in proportion to the number of candles; two candles giving twice the rapidity of one candle, and three, three times, &c.

The position of the light in the horizontal plane of the instrument is of no consequence, provided the distance is not altered; thus two candles, one foot off, give the same number of revolutions per second, whether they are side by side or opposite to each other. From this it follows that if the radiometer is brought into a uniformly lighted space it will continue to revolve.

It is easy to get rotation in a radiometer without having the surfaces of the discs differently coloured. Here is one having the pith discs blacked on both sides. I project its image on the screen, and there is no movement. I bring a candle near it, and shade the light from one side, when rapid rotation is produced, which is at once altered in direction by moving the shade to the other side.

I have arranged here a radiometer so that it can be made to move by a very faint light, and at the same time its rotation is easily followed by all present. In this bulb is a large six-armed radiometer carrying a mirror in its centre. The mirror is almost horizontal, but not quite so, and therefore when I throw a beam of electric light vertically downwards on to the central mirror, the light is reflected off at a slight angle, and as the instrument rotates its movement is shown by the spot of light travelling round the ceiling in a circle. Here again the fog helps us, for it gives us an imponderable beam of light moving round the room like a solid body, and saving you the trouble of looking up to the ceiling. I now set the radiometer moving round by the light of a candle, and I want to show you that coloured light does not very much interfere with the movement. I place yellow glass in front, and the movement is scarcely diminished at all. Very deep coloured glass, you see, diminishes it a little more. Blue and green glass make it go a little slower, but still do not diminish the speed one-half. I now place a screen of water in front: the instrument moves with diminished velocity, rotating with about one-fourth its original speed.

Taking the action produced by a candle flame as 100

Yellow glass reduces it to . . .	89
Red ,, ,, ,, . . .	71
Blue ,, ,, ,, . . .	56
Green ,, ,, ,, . . .	56
Water ,, ,, . . .	26
Alum ,, ,, . . .	15

I now move the candle a little distance off, so as to make the instrument move slower, and bring a flask of boiling water close to it. See what happens. The luminous index no longer moves steadily, but in jerks. Each disc appears to come up to the boiling water with difficulty, and to hurry past it. More and more sluggishly do they move past, until now one has failed to get by, and the luminous beam, after oscillating to and fro a few times, comes to rest. I now gradually bring the candle near. The index shows no movement. Nearer still. There is now a commencement of motion, as if the radiometer was trying to push past the resistance offered by the hot water; but it is not until I have brought the candle to within a few inches of the glass globe that rotation is recommenced. On these pith radiometers the action of dark heat is to repel the black and white surfaces almost equally, and this repulsion is so energetic as to overcome the rotation caused by the candle, and to stop the instrument.

With a radiometer constructed of a good conductor of heat, such as metal, the action of dark heat is different. Here is one made of silvered copper, polished on one side and lampblacked on the other. I have set it moving with a candle slightly the normal way. Here is a glass shade heated so that it feels decidedly warm to the hand. I cover the radiometer with it, and the rotation first stops, and then recommences the reverse way. On removing the hot shade the reverse movement ceases, and normal rotation recommences.

If, however, I place a hot glass shade over a pith radiometer the arms at once revolve the normal way, as if I had exposed the instrument to light. The diametrically opposite behaviour of a pith and a metal instrument when exposed to the dark heat radiated from a hot glass shade is very striking. The explanation of the action is not easy, but it depends on the fact that the metal is one of the best conductors of heat, whilst pith is one of the worst.

One more experiment with this metallic radiometer. I heat it strongly with a spirit-lamp, and the arms spin round rapidly. Now the whole bulb is hot, and I remove the lamp: see what happens. The rotation quickly diminishes. Now it is at rest; and now it is spinning round just as fast the reverse way. I can produce this reverse movement only with difficulty with a pith instrument. The action is due to the metal being a good conductor of heat. As it absorbs heat it moves one way; as it radiates heat it moves the opposite way.

At first I made these instruments of the very lightest material possible, some of them not weighing more than half a grain; and where extreme sensitiveness is required lightness is essential. But the force which carries them round is quite strong enough to move a much greater weight. Thus the metallic instrument I have just experimented with weighs over 13 grains, and here is one still heavier, made of four pieces of looking-glass blacked on the silvered side, which are quickly sent round by the impact of this imponderable agent, and flash the rays of light all round the room when the electric lamp is turned on the instrument.

Before dismissing this instrument let me show one more experiment. I place the looking-glass and the metal radiometer side by side, and, screening the light from them, they come almost to rest. Their temperature is the same as that of the room. What will happen if I suddenly chill them? I pour a few drops of ether on each of the bulbs. Both instruments begin to revolve. But notice the difference. Whilst the movement in the case of the metal radiometer is direct, that of the looking-glass instrument is reverse. And yet to a candle they both rotate the same way, the black being repelled.

Now, having found that this force would carry round a comparatively heavy weight, another useful application suggested itself. If I can carry round heavy mirrors or plates of copper, I can carry round a magnet. Here, then (Fig. 9), is an instrument carrying a magnet, and outside is a smaller magnet, delicately balanced in a vertical position, having the south pole at the top and the north pole at the bottom. As the inside magnet comes round, the outside magnet, being delicately suspended on its centre, bows backwards and forwards, and, making contact at the bottom, carries an electric current from a battery to a Morse instrument. A ribbon of paper is drawn through the "Morse" by clockwork, and at each contact—at each revolution of the radiometer—a record is printed on the strip of paper by dots; close together if the radiometer revolves quickly, farther apart if it goes slower.

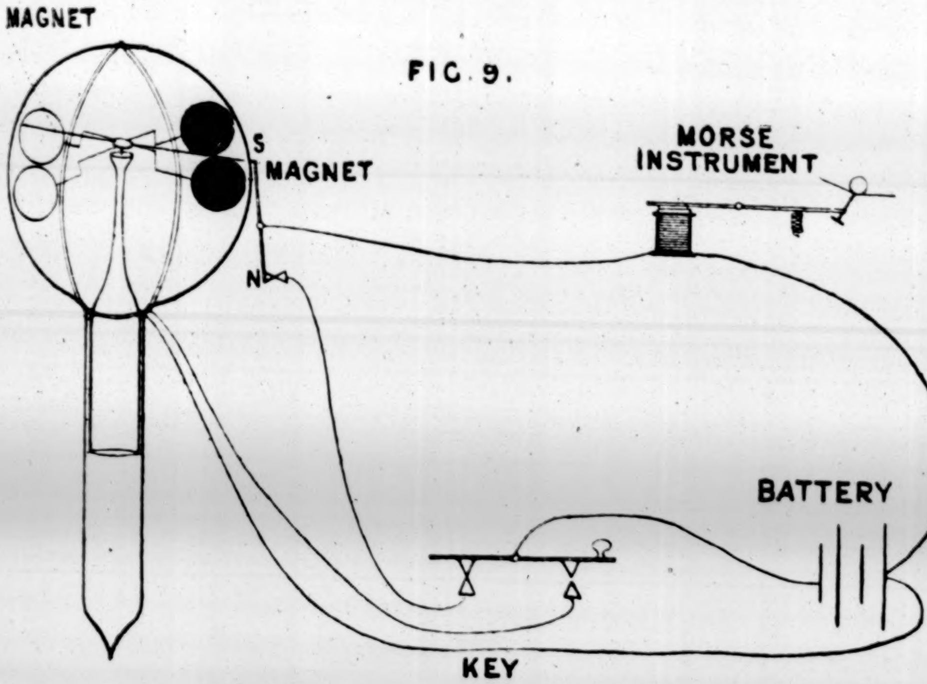
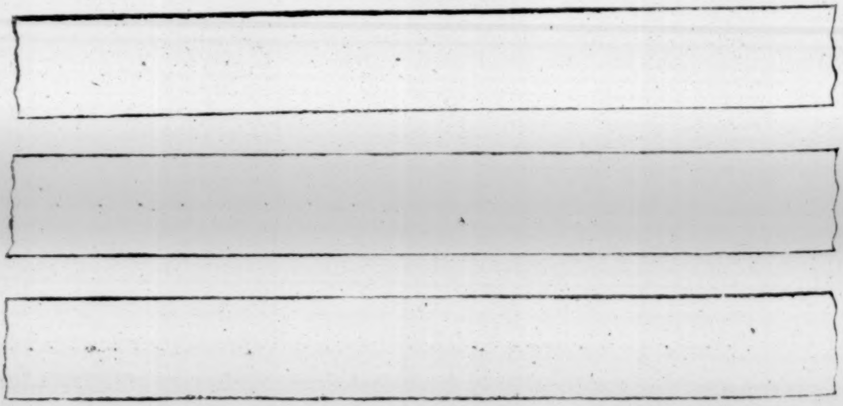


FIG. 9.

Here the inner magnet is too strong to allow the radiometer to start with a faint light without some initial impetus. Imagine the instrument to be on the top of a mountain away from everybody, and I wish to start it in the morning. Outside the bulb are a few coils of insulated copper wire, and by depressing the key for an instant I pass an electric current from the battery through them. The interior magnet is immediately deflected from its north-south position, and the impetus thus gained enables the light to keep up the rotation. In a proper meteorological instrument I should have an astatic combination inside the bulb, so that a very faint light would be sufficient to start it, but in this case I am obliged to set it going by an electric current. I have placed a candle near the magnetic radiometer. I now touch the key; the instrument immediately responds; the paper unwinds from the Morse instrument, and on it you will see dots in regular order. I put the candle 8 inches off, and the dots come wide apart. I place it $5\frac{3}{4}$ inches off, and two dots come where one did before. I bring the candle 4 inches from the instrument, and the dots become four times as numerous (Fig. 10), thus recording automatically the intensity of the light falling on the instrument, and proving that in this case also the radiometer obeys the law of inverse squares.

This instrument, the principle of which I have illustrated to-night, is not a mere toy or scientific curiosity, but it is capable of giving much useful information in climatology. You are well aware that the temperature, the rainfall, the atmospheric pressure, the direction and force of the wind, are now carefully studied in most countries, in order to elucidate their sanitary condition, their animal and vegetable productions, and their agricultural capabilities. But one most important element, the amount of light received at any given place, has been hitherto but very crudely and approximately estimated, or rather guessed at. Yet it cannot be denied that sunlight has its effect upon life and health, vegetable, animal, and human, and that its relative amount at any place is hence a point of no small moment. The difficulty is now overcome by such an instrument as this. The radiometer may be permanently placed on some tall building, or high mountain, and, by connecting it by

FIG. 10.



telegraphic wires to a central observatory, an exact account can be kept of the proportion of sunlight received in different latitudes, and at various heights above the sea-level. Furthermore, our records of the comparative temperature of different places have been hitherto deficient. The temperature of a country depends partly on the amount of rays which it receives direct from the sun, and partly on the atmospheric and oceanic currents, warm or cold, which sweep over or near it. The thermometer does not discriminate between these influences; but the radiometer will enable us now to distinguish how much of the annual temperature of a place is due to the direct influence of the sun alone, and how much to the other factors above referred to.

I now come to the last question which I stated at the beginning of this lecture—"What is the amount of force exerted by radiation?" Well, I can calculate out the force in a certain way, from data supplied by this torsion apparatus (Fig. 4). Knowing the weight of the beam, the power of the torsion fibre of glass, its time of oscillation, and the size of the surface acted on, it is not difficult to calculate the amount of force required to deflect the beam through a given angle; but I want to get a more direct measure of the force. I throw a ray of light upon one of these instruments, and it gives a push; surely it is possible to measure the amount of this push in parts of a grain. This I have succeeded in doing in the instrument behind me; but before showing the experiment I want to illustrate the principle upon which it depends. Here is a very fine glass fibre suspended from a horizontal bar, and I wish to show you the strength of it. The fibre is only a few thousandths of an inch thick; it is about 3 feet long, and at the lower end is hanging a scale-pan, weighing 100 grains. So I start with a pull of 100 grains on it. I now add little lead weights, 50 grains each, till it breaks. It bears a pull of 750 grains, but gives way when additional weight is added. You see then the great strength of a fibre of glass, so fine as to be invisible to all who are not close to it, to resist a tensile strain.

Now I will illustrate another equally important property of a glass thread, viz., its power to resist torsion. Here is a still finer glass thread, stretched horizontally between two supports; and in order to show its position I have put little jockeys of paper on it. One end is cemented firmly to a wooden block, and the other end is attached to a little instrument called a counter—a little machine for registering the number of revolutions. I now turn this handle till the fibre breaks, and the counter will tell me how many twists I have given this fibre of glass. You see it breaks at twenty revolutions. This is rather a thicker fibre than usual. I have had them bear more than 200 turns without breaking, and some that I have worked with are so fine that if I hold one of them by the end it curls itself up and floats about the room like a piece of spider's thread.

Having now illustrated these properties of glass fibres, I will try to show a very delicate experiment. I want to ascertain the amount of pressure which radiation exerts on a blackened surface. I will put a ray of light on the pan of a balance, and give you its weight in grains, for I think in this Institution and before this audience I may be allowed a scientific use of the imagination, and may speak of weighing that which is not affected by gravitation.

The principle of the instrument is that of W. Ritchie's torsion balance, described by him in the "Philosophical Transactions" for 1830. The construction is somewhat complicated, but it can be made out on reference to the diagram (Fig. 11). A light beam, A B, having 2 square inches of pith, C, at one end, is balanced on a very fine fibre of glass, D D′, stretched horizontally in a tube; one end of the fibre being connected with a torsion handle, E, passing through the tube, and indicating angular movements on a graduated circle. The beam is cemented to the torsion fibre, and the whole is enclosed in glass and connected with the mercury pump by a spiral tube, F, and exhausted as perfectly as possible. G is a spiral spring, to keep the fibre in a uniform state of tension. H is a piece of cocoon silk. I is a glass stopper, which is ground into the tube as perfectly as possible, and then highly polished and lubricated with melted india-rubber, which is the only substance I know that allows perfect lubrication and will still hold a vacuum. The pith, C, represents the scale-pan of the balance. The cross-beam, A B, which carries it, is cemented firmly to the thin glass fibre, D, and in the centre is a piece of mirror, K. Now the cross-beam A B and the fibre D being rigidly connected together, any twist which I give to the torsion handle E will throw the beam out of adjustment. If, on the other hand, I place a weight on the piece of pith C, that end of the beam will fall down, and I shall have to turn the handle, E, round and round a certain number of times, until I have put sufficient torsion on the fibre D to lift up the beam. Now, according to the law of

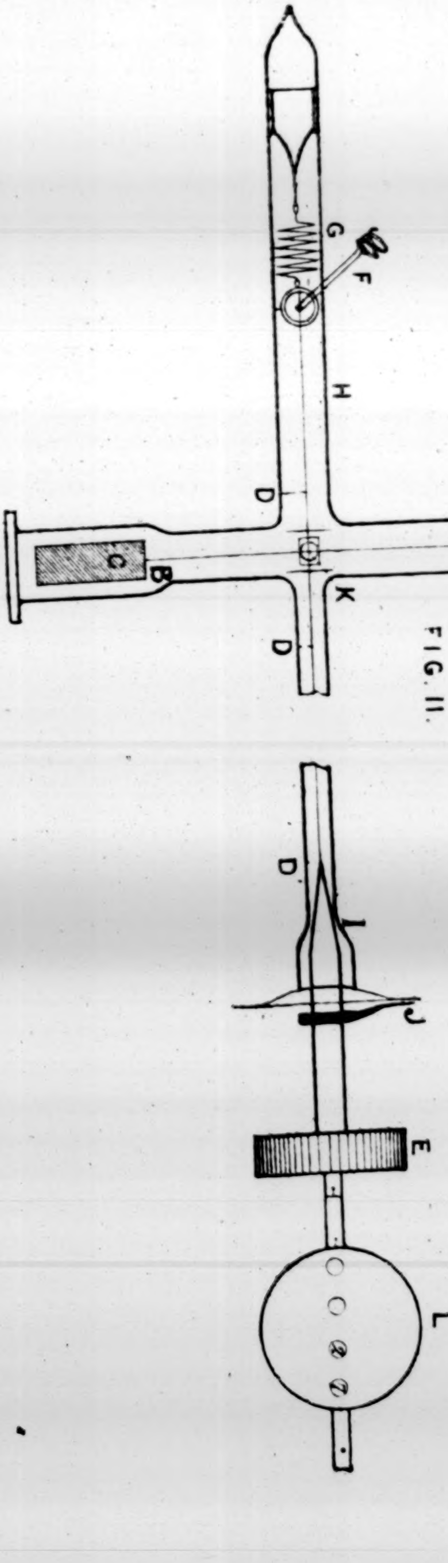


FIG. II.

torsion, the force with which a perfectly elastic body like glass tends to untwist itself is directly proportional to the number of degrees through which it has been twisted; therefore, knowing how many degrees of torsion I must put on the fibre to lift up the 1-100th of a grain weight, I can tell how many degrees of torsion are required to lift up any other weight; and conversely, putting an unknown weight or pressure on the pith, I can find its equivalent in grains by seeing how much torsion it is equal to. Thus, if 1-100th of a grain requires 10,000 degrees of torsion, 1-50th of a grain would require 20,000 degrees; and conversely, a weight which required 5000 degrees torsion would weigh 1-200th of a grain. Once knowing the torsion equivalent of 1-100th of a grain, the ratio of the known to the unknown weights is given by the degrees of torsion.

Having thus explained the working of the torsion balance I will proceed to the actual experiment. On the central mirror I throw a ray from the electric light, and the beam reflected on a particular spot of the ceiling will represent zero. The graduated circle J of the instrument also stands at zero, and the counter which I fasten on at the end L stands at 0. The position of the spot of light reflected from the little concave mirror being noted, the torsion balance enables me to estimate the pressure or weight of a beam of light to a surprising degree of exactness. I lift up my little iron weight by means of a magnet (for working in a vacuum I am restricted in the means of manipulating), and drop it in the centre of the pith: it knocks the scale-pan down, as if I had placed a pound weight upon an ordinary balance, and the index ray of light has flown far from the zero-point on the ceiling. I now put torsion on the fibre to bring the beam again into equilibrium. The index-ray is moving slowly back again. At last it is at zero, and on looking at the circle and counter I see that I have had to make 27 complete revolutions and 301 degrees, or $27 \times 360^\circ + 301^\circ = 10{,}021^\circ$, before the force of torsion would balance the 1-100th of a grain.

I now remove the weight from the pith-pan of my balance, and liberate the glass thread from torsion by twisting it back again. Now the spot of light on the ceiling is at zero, and the counter and index are again at 0.

Having thus obtained the value of the 1-100th of a grain in torsion degrees, I will get the same for the radiation from a candle. I place a lighted candle exactly 6 inches from the blackened surface, and on removing the screen the pith scale-pan falls down, and the index-ray again flies across the ceiling. I now turn the torsion handle, and in much less time than in the former case the ray is brought back to zero. On looking at the counter I find it registers four revolutions, and the index points to 188 degrees, making altogether $360° \times 4 + 188 = 1628°$, through which the torsion fibre has to be twisted to balance the light of the candle.

It is an easy calculation to convert this into parts of a grain weight; 10,021 torsion degrees representing 0·01 grain, 1628 torsion degrees represent 0·001624 grain.

$$10{,}021° : 0{\cdot}01 \text{ grain} :: 1628° : 0{\cdot}001624 \text{ grain}.$$

The radiation of a candle 6 inches off, therefore, weighs or presses the two square inches of blackened pith with a weight of 0·001624 grain. In my own laboratory, working with this torsion balance, I found that a candle 6 inches off gave a pressure of 0·001772 grain. The difference is only 0·000148 grain, and is fairly within the allowable limits of a lecture experiment. But this balance is capable of weighing to far greater accuracy than that. You have seen that a torsion of 10,021° balanced the hundredth of a grain. If I give the fibre 1 degree more twist the weight is over-balanced, as shown by the movement of the index-ray on the ceiling. Now 1 degree of torsion is about the 1-10,000th part of the whole torsion required by the 1-100th grain. It represents therefore the 1-10,000th part of the 1-100th, or the millionth part of a grain.

Divide a grain weight into a million parts, place one of them on the pan of the balance, and the beam will be instantly depressed!

Weighed in this balance the mechanical force of a candle 12 inches off was found to be 0·000444 grain; of a candle 6 inches off, 0·001772 grain. At half the distance the weight of radiation should be four times, or 0·001776 grain; the difference between theory and experiment being only four-millionths of a grain is a sufficient proof that the indications of this instrument, like those of the apparatus previously described, follow the law of inverse squares. An examination of the differences between the separate observations and the mean shows that my estimate of the sensitiveness of this balance is not excessive, and that in practice it will safely indicate the millionth of a grain.

I have only had one opportunity of getting an observation of the weight of sunlight: it was taken on December 13th, but the sun was so obscured by thin clouds and haze that it was only equal to 10·2 candles 6 inches off. Calculating from this datum, it is seen that the pressure of sunshine is 2·3 tons per square mile.

But however fair an equivalent ten candles may be for a London sun in December, a midsummer sun in a cloudless sky has a very different value. Authorities differ as to its exact equivalent, but I under-estimate it at 1000 candles 12 inches off.

Let us see what pressure this will give:—A candle 12 inches off, acting on 2 square inches of surface, was found equal to 0·000444 grain; the sun, equalling 1000 candles, therefore gives a pressure of 0·444000 grain; that is equal to about 32 grains per square foot, to 2 cwts. per acre, 57 tons per square mile, or nearly three thousand million tons on the exposed surface of the globe—sufficient to knock the earth out of its orbit if it came upon it suddenly.

It may be said that a force like this must alter our ordinary ideas of gravitation; but it must be remembered that we only know the force of gravity as between bodies such as they actually exist, and we do not know what this force would be if the temperatures of the gravitating masses were to undergo a change. If the sun is gradually cooling, possibly its attractive force is increasing, but the rate will be so slow that it will probably not be detected by our present means of research.

Whilst showing this experiment I wish to have it distinctly understood that I do not attach the least importance to the actual numerical results. I simply wish to show you the marvellous sensitiveness of the apparatus with which I am accustomed to work. I may, indeed, say that I know these rough estimates to be incorrect. It must be remembered that our earth is not a lamp-blacked body enclosed in a glass case, nor is its shape such as to give the maximum of surface with the minimum of weight. The solar forces which perpetually pour on it are not simply absorbed and degraded into radiant heat, but are transformed into the various forms of motion we see around us, and into the countless forms of vegetal, animal, and human activity. The earth, it is true, is poised in vacuous space, but it is surrounded by a cushion of air; and, knowing how strongly a little air stops the movement of repulsion, it is easy to conceive that the sun's radiation through this atmospheric layer may not produce any important amount of repulsion. It is true the upper surface of our atmosphere must present a very cold front, and this might suffer repulsion by the sun; but I have said enough to show how utterly in the dark we are as to the cosmical bearings of this action of radiation, and further speculation would be but waste of time.

It may be of interest to compare these experimental results with a calculation made in 1873, before any knowledge of these facts had been made public.

Prof. Clerk Maxwell, in his "Electricity and Magnetism," vol. ii., p. 391, writes as follows:—"The mean energy in one cubic foot of sunlight is about 0·0000000882 of a foot-pound, and the mean pressure on a square foot is 0·0000000882 of a pound weight. A flat body exposed to sunlight would experience this pressure on its illuminated side only, and would therefore be repelled from the side on which the light falls."

Calculated out, this gives the pressure of sunlight equal to about 2½ lbs. per square mile. Between the 2½ lbs. deduced from calculation and the 57 tons obtained from experiment the difference is great; but not greater than is often the case between theory and experiment.

In conclusion, I beg to call especial attention to one not unimportant lesson which may be gathered from this discovery. It will be at once seen that the whole springs from the investigation of an anomaly. Such a result is by no means singular. Anomalies may be regarded as the finger-posts along the high road of research, pointing to the bye-ways which lead to further discoveries. As scientific men are well aware, our way of accounting for any given phenomenon is not always perfect. Some point is perhaps taken for granted, some peculiar circumstance is overlooked. Or else our explanation agrees with the facts not perfectly, but merely in an approximate manner, leaving a something still to be accounted for. Now these residual phenomena, these very anomalies, may become the guides to new and important revelations.

In the course of my research anomalies have sprung up in every direction. I have felt like a traveller navigating some mighty river in an unexplored continent. I have seen to the right and the left other channels opening out, all claiming investigation, and promising rich rewards of discovery for the explorer who shall trace them to their source. Time has not allowed me to undertake the whole of a task so vast and so manifold. I have felt compelled to follow out, as far as lay in my power, my original idea, passing over reluctantly the collateral questions springing up on either hand. To these I must now invite the attention of my fellow-workers in Science. There is ample room for many enquirers.

Nor must we forget that the more rigidly we scrutinise our received theories, our routine explanations, and interpretations of Nature, and the more frankly we admit their shortcomings, the greater will be our ultimate reward. In the practical world fortunes have been realised from the careful examination of what has been ignorantly thrown aside as refuse; no less, in the sphere of Science, are reputations to be made by the patient investigation of anomalies.

NOTICES OF BOOKS.

Christian Psychology : the Soul and the Body in their Correlation and Contrast. Being a New Translation of Swedenborg's Tractate "*De Commercio Animi et Corporis.*" With Preface and Illustrative Notes. By T. M. GORMAN, M.A. London : Longmans and Co.

IT is a subject which might be debated at great length in how far the present work can legitimately come under our cognisance. The translator, indeed, assures us in his preface that the contents of the original work of Swedenborg are "of a strictly philosophical character," but he immediately adds "although it evidently contains several distinct indications of having been written with a definite theological aim." The work before us, however, to 113 pages due to Swedenborg himself, contains upwards of 400 for which Mr. Gorman must be held responsible, aad in which the theological element and the theological style are still more pronounced. In the preface is a vigorous attack upon a certain Vicar of Frome-Selwood, and a still fiercer onslaught upon Cardinal Manning. There is a protest warning the world not in any way to confound Swedenborg with the Swedenborgians, who are described as "the small sect founded in the year 1787, under most unhappy auspices, by a certain Robert Hindmarsh, of Clerkenwell Close, a layman, and formerly a member of Mr. Wesley's Communion." In all this we can see very little of philosophical interest.

The main purpose of the Appendix is declared to be "to furnish further illustration of the text, and to indicate a few relations of agreement or opposition which appear to exist between certain speculations which have obtained considerable currency in the present day, and some of the more important principles taught in our author's writings more than a century ago. This may, perhaps, be a fitting place to attempt the difficult task of furnishing an estimate of Swedenborg's character as a man of Science. Years ago we took up certain of his writings—especially a treatise on chemistry—with a very strong prepossession, not against him, but in his favour. We had heard it declared that Swedenborg had all but anticipated the Atomic Theory, and that his works teemed with valuable suggestions. We read accordingly, and re-read, eagerly and carefully. But the more we read the more we were disappointed. What we sought was not there. There was nothing to point the path to new regions of discovery ; there were no ideas, at least to our comprehension, which admitted of experimental verification. Reluctantly we laid the book aside, doubting whether the fault lay with the author or with ourself. But when we came upon the "Philosophie Chimique" of Dumas we found his estimate of Swedenborg as a chemist agree very closely with our own. Since that time we have watched the gradual development of chemical science; we have taken part in the attack and defence of theories and systems, but we have not succeeded in detecting, in the course of modern discovery, anything which is fairly due to the inspiration of Swedenborg, or of which his writings can be considered the fountain. At the same time we do not dispute that a subtle and eager partisan might imagine that he saw in the fruits of modern research a fulfilment of some of the foreshadowings of this remarkable man, and, from the peculiar nature of Swedenborg's writings, it might be difficult to convince such an enthusiast of error. As regards his astronomical speculations, and in particular the knowledge which he fancied he had received in visions concerning some of the heavenly bodies, we cannot do better than remind the reader of the point so ably brought forward by the authors of the "Unseen Universe." These gentlemen call attention to the significant fact that whilst Swedenborg gave the world an account of the planets whose existence was then known to the educated world, he is perfectly silent concerning Uranus and Neptune, which have been discovered since his time. Had he pointed out their existence he would have fully legitimised his claims to extraordinary knowledge, howsoever obtained. His having failed to do so must be considered as fatal.

As a specimen of Swedenborg's scientific writings we will quote, from his "Economy of the Animal Kingdom," a passage which Mr. Gorman characterises as "magnificent," and which he really seems to imagine is an answer to and an anticipatory refutation of Dr. Huxley's well-known work "Man's Place in Nature":—"It is especially to be remarked that all the wills and actions of animals—we mean all the instincts—are excited simply by external motives or moving causes, by those things that strike the senses, or that affect their blood in a general manner. The changes and conditions of the air and æther recurring with the four seasons send heat into their fluids, which burn and boil accordingly (!), and with the fluids as determinants a corresponding change is wrought in the organic forms of the body and brain. In this way the principle of motion is at once excited, and animals are carried agreeably to Nature's order into rational-seeming effects involving ends. Hence their loves, and hence the periods those loves obey. Hence the wonders they display in building their nests, incubating their eggs, and hatching their young. Hence their amazing parental care. Hence their public consultations as to the manner of providing for themselves and their progeny in the coming winter; and a number of other effects which proceed from a soul like theirs, accommodated to the reception of life according to its own peculiar character, whenever it is excited by appropriate circumstances. Experience attests the truth of these remarks. For we know that the same effects are produced on animals by the warmth or heat of a room as by the heat of the sun, and when the season is neither spring nor summer. We may therefore say that the soul of animals resides in their blood, because it is always actuated by a cause extrinsic to itself. Not so the soul of man. He indeed is likewise moved, yet he is not governed by external causes. The affections of the external world pass *a posteriori* in some measure into the sphere of intelligence, yet in the man himself they are determined into act by a foregone will arising from an appropriate principle and cause. Thus we men are stirred to action by a fire kindled in the very sphere of the (intellectual) mind, even in mid-winter. As the philosopher (Aristotle) says:—'Whatever a secondary cause can do, a prior cause can also do in a higher and more noble manner. The first cause assists the second in its operations, and secondary causes are illuminated by the light of the first.' O! then, how obscured —how deeply buried in the grave of the body are the minds of those who judge of themselves by the brutes, and of their own souls by the souls of brutes, reasoning from likeness of actions, likeness of senses, and likeness of brain so far as the eye alone discloses the brain; and do not see beyond the likeness, nor how far we stand apart from them; fit subjects, indeed, for ridicule, did they not rather deserve our pity."

Now this magnificent passage may perhaps be rhetoric, though plain-spoken persons would probably consider it mere "padding." But it is assuredly not Science. It is what no earnest worker in Science could ever have written. The more closely we study it the less we feel disposed to expect anything of value from its author. In further exemplification of the same view we may refer to another passage on "the apparent resemblance and absolute difference between the brain of man and that of brute animals," and to a dissertation on the "essential distinction between man and brutes." Swedenborg—like his disciple and interpreter Mr. Gorman, and indeed like most men who study Nature in books, or in their own imaginings, instead of in things —was a profound believer in the absolute distinction and immense interval between man and "brutes." Our author accordingly does his poor best to be witty at the expense of Dr. Huxley. Says he—"The self-confident advocates of the 'brute' view of man's nature and origin cannot reasonably be offended if they are taken more or less at their word in this matter, albeit in a sense widely different from that intended by some who indulge their humour for writing 'Lay Sermons' for the enlightenment and edification of British working-men." The advocates of the doctrine of Evolution have more important duties in hand than feeling offended at Mr. Gorman's "takes" or mistakes. But we may remind him that it is not the best and noblest man who is most ashamed of his poor relations.

We are here reminded of a further piece of evidence which proves that the work before us is in its spirit theological rather than philosophical. We refer to the manner in which Mr. Gorman treats all dissidents. He is not one of those who can agree to differ. He cannot apparently believe it possible for conscientious and enlightened minds to arrive at conclusions other than his own. All such he calls, in fact, with but little circumlocution, either knaves or fools, and imputes to them the most unworthy motives. No inconsiderable portion of the book is made up of such elegancies as the following:—"Meanest and most malignant subterfuges," "bestial fabrication," "wicked forgery," "offensive and slanderous epithets," "intolerable social nuisance," "flagitious example," "audacious and mischievous attempt," "wanton and profane hostility," "narrow-minded and arrogant class of intellectual obstructives," "the dark, narrow, tortuous, intolerant, and earthy tone and temper of Dr. Priestley's lucubrations," "the pseudo-scientific sect of Darwinian evolutionists," &c. Do not let the author, however, flatter himself that he can succeed even in offending the eminent men whom he denounces. They will give him that contemptuous pardon which is the privilege of a certain class. Says a German poet—

"What cares the moon
If a dog barks at her?"

If he wishes to criticise men of Science we would advise him to spend some eight or ten years at genuine scientific work, as a needful preliminary. He would then see not a few things in a very different light.

Thermo-Dynamical Phenomena, or the Origin and Physical Doctrine of "Life," and the New Theory of "Fermentation." By H. A. Huntley. Madras: Foster and Co. London: Longmans, Green, and Co.

The title of this pamphlet scarcely gives a full and fair idea of the author's objects. We quote, therefore, his opening paragraph:—

"The much-agitated question of the origin of 'Life' from organic liquids—or the spontaneous generation of Infusorial Animalculæ, Monad, Bacterium, and Vibrio, &c.—has always engaged the attention of careful observers; but owing to the indefiniteness (and hence conflicting character) of the results advanced no true scientific conclusion has hitherto been drawn. Moreover, a majority of scientific men having been engrossed and subsequently gratified with the impulsive deduction drawn by Virchow —'that all known facts are opposed to the said theory of Generatio Spontanea;' and the remainder being absorbed by the vagary of the 'Germ theory' extant, proclaiming '*omne vivum ex vivo*;' analogous to the settled conclusions anent the natural processes of development of the sub-kingdom Vertebrata, Mollusca, &c., which patient investigation elucidated with regard to the origin of these; the real *cause* of 'Generatio Spontanea' has therefore been absolutely evaded, and hitherto involved in obscurity."

From this passage—which we can scarcely say gives us a very favourable impression of the author's habits of thought—we learn that he upholds the doctrine of Abiogenesis, and that he denies or doubts the presence, in the atmosphere and elsewhere, of organic germs, which under suitable circumstances may be developed. He repeatedly puts before us the question—"What is the real cause of Generatio Spontanea?—what is the cause of the development of these Infusorial Animalculæ from organic matter?" But before inquiring into the cause, it would surely be wise to be certain of the existence of the effect. This we do not see that our author succeeds in doing. We find no experiments here recorded where vegetal or animal life has arisen in solutions placed under such circumstances that no germs or seeds could have been introduced from without. Instead of such he tells us:—

"This conclusion which has been arrived at, of *heat* being the *cause* of spontaneous generation, has been ascertained by the following series of experiments, conducted, as will be noticed, with the most careful observation:—

"I. Any organic substance, either vegetable or animal, or both together, without previous boiling, subjected to the influence of and for the absorption of a *legitimate* temperature of heat (a subject I shall subsequently demonstrate), resulted in the production of living Animalculæ.

"II. I subjected a vegetable solution in cold water (without boiling), in a vessel, to a legitimate temperature of heat: after the lapse of some hours microscopic investigation evinced signs of active vivacity; in other words, Infusorial Animalculæ were skating about numerously, in active vivacity."

Now, that the author actually obtained these results our readers will scarcely question. No precautions were taken to destroy pre-existing life by boiling, or to prevent its subsequent introduction from without. By way of demonstration that the organisms developed in the liquid were not occasioned by "germs," we find the following passage:—"To prove my assertion that the said *germs* are *visionary* I cannot do better than quote Dr. Dougall, who recently asked—'What must be the size of the *germs* which give rise to *organisms* of about 1,400,000th of an inch, and *less*, in diameter? and, further, if the said germs bear the same proportion to their adults as that which obtains between the *germs* of plants or animals familiar to us, and those when fully developed, the difference in some cases may amount to no more than tens of thousands, but in other cases it must be hundreds of millions. Compare, for example, an acorn with an oak; a turnip-seed to the large succulent bulb (!) it produces; the human *germ*, measuring about the 250th part of an inch, to a man weighing 16 stone; and it will be obvious that the *germs* of organisms in putrefying solutions, *if such exist*, are minute beyond comprehension, and that the *highest powers* of the *microscope* must ever be immeasurably inadequate to detect their presence.' These sentiments alone are enough to refute the theory *omne vivum ex vivo*, as regards the origin of the Infusoria and the lowest Fungi, as mould, or mildew, &c." With all due deference to the author and to Dr. Dougall, we cannot help regarding the man who can deny the existence of germs, on the strength of such considerations, a psychological curiosity. To doubt or deny the existence of anything because to our faculties or to our instruments it seems minute is an outcome of that vicious old principle which seeks to make man the measure of all things. The fact so often urged by the opponents of Abiogenesis,—that sealed tins of milk, meat, soups, &c., are not when opened found swarming with animal and vegetable life,—the author seeks to explain by saying that they "had not arrived at that stage to absorb a legitimate heat and result in life." Such phraseology seems to us the very essence of "vagueness." "A boiling heat or a frozen atmosphere are temperatures illegitimate and unregistered." Why the boiling heat should be called "unregistered," or what is the precise meaning of that term, we do not profess to decipher. But letting that pass; if "legitimate heat" denotes the temperatures below the boiling, but above the freezing point, we should think that a tin of meat, after being sealed and boiled in Australia, must, during its passage to England, have had every opportunity of absorbing "legitimate heat" enough. Yet we never hear of the development of Bacteria, &c., in the tins, except there is some breach of continuity admitting the outward air.

We do not see that Mr. Huntley has succeeded in throwing any new light on the very important and difficult question with which he has attempted to grapple.

Light as a Motive Power: a Series of Meteorological Essays.

By Lieut. R. H. Armit, R.N. Vol. I. London: J. D. Potter.

The author announces his intention of proving that "there is only one law, one life, and one death in Nature, and that her *primum mobile* is the universally felt power of light." He will also "endeavour to reconcile scientific discoveries with the account given in the Bible of all natural phenomena." As might be expected, from this declaration, we find in the book not a few remarkable statements. Thus the history of electricity, we are told, "dates back to the creation of the world." Electricity he seems to view not as a mode of force, but as a "fluid" capable of being decomposed into two elements. In the first Essay, entitled "A Retrospective Glance at Meteorology," we are particularly struck with the following passage:—"Whatever direction future inquiry may take it will ever be found, by the student of meteorology, that the Old Testament Scriptures contain more valuable data to go upon, leading nearer to the actual origin and cause of meteorological phenomena, than any other book in existence, *but the allusions to the laws of Nature, their operations, and effects, as contained in the Bible, are so masked and wrapped up in peculiar phraseology that the meaning, though peeping out all the while, yet lies concealed, and remains so until the light of Science is thrown upon it*, when it bursts out and strikes us with remarkable force, causing us to pause and ponder, and then to ask ourselves whether it be not true that 'that which hath been, it is that which shall be?'" We beg to call the especial attention of the reader to the words we have italicised, and which are, substantially, a confession that those men who receive the Bible as a physical revelation see in it whatever they wish, and in fact "read between the lines." This is a truth fully demonstrated by the history of Science. When the geocentric theory of the universe was still in vogue, divines—Protestant* no less than Catholic—proclaimed it to be a scriptural truth. When the Copernican theory triumphed, it too was held to be shadowed forth in the Bible. Even the nebular theory of Kant and Laplace, so long denounced as atheistic, is now declared—by no lower authorities than the authors of the "Unseen Universe"—to be involved in the sacred volume.

Perusing further this "Retrospect," we read of the Ethiopian merchants, who visited Judæa of old, sitting in an evening "enjoying the cool breeze, and, whilst *smoking their chibouk*, narrating the dangers through which they had passed in collecting those very goods that they had come to Palestine to sell, and thus instruct the Israelites." We were certainly not aware that the use of tobacco was at all known to the ancient Hebrews. Another historical mis-statement is to be found in the same section:—"After conquering the Gauls, the Roman legions under Cæsar invaded *Anglia*, as England was then called." The name Anglia was not applied to any part of Britain until after the withdrawal of the Romans.

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* For instance, Dean Wren, the father of the distinguished Sir Christopher Wren.

Leaving, however, such minor points, we turn to the author's fundamental proposition that our globe is "surrounded by a transparent, spherical, metallic shell, enclosing within its folds the rising vapours from the earth, and preventing them from flying off into space, in the same way that the boiler-plates of an ordinary boiler enclose the steam which works the whole machinery." We naturally ask for the evidence upon which so extraordinary a statement is based. In reply, we are told that the atmosphere contains metallic gases, due to some obscure process of evaporation, constantly going on, by which all mineral bodies are more or less affected throughout the whole earth and the metallic vapour thus evolved—finding its way into the atmosphere—is thereby absorbed: the heat which creates the constant mineral volatilisation that turns the upper currents of our atmosphere into a metallic gas is derived not only from the sun, but also from the state of combustion in which it is believed the centre of the earth exists. Heat applied to water produces steam, which is absorbed by the atmosphere, and we have only to enter any foundry to see metallic vapour being likewise so absorbed. Under such circumstances it is only natural to conclude that all the minerals volatilised from a combination of causes find their way into our atmosphere. If they do not, whence come the aërolites which Nature precipitates at our feet, and which all reach the earth in the shape of boulders of highly-magnetised pure metal." Now, we admit the presence of water in the atmosphere, because we have direct proof of its existence. If "all minerals" are volatilised in a similar manner, their presence should be capable of demonstration. It would surely be sufficient to draw some thousand cubic metres of air by means of an aspirator through different solutions, such as hydrochloric acid, hydrosulphate of ammonia, &c.,—taking the precaution to exclude dust by a plug of cotton-wool at the entrance end,—and then subjecting the liquids to a careful examination. That at certain temperatures all metals must be volatilised we are not disposed to doubt; but experience shows that as soon as the temperature is reduced below a certain point, these metallic vapours are re-condensed. Even if this were not the case, why should these supposed gaseous metals collect in the upper regions of the atmosphere? In what state or combination would they have to exist so as to be perfectly transparent and diathermanous, and yet be insoluble in the atmosphere? The meteorolites consist chiefly of iron, cobalt, and nickel,—the first-mentioned being widely diffused, indeed, but requiring a very high temperature for its volatilisation; the other two likewise very fixed, and decidedly rare. Why do we not find aërolites of the more volatile metals, such as lead or zinc? As soon as Lieutenant Armit, or any one else, shall demonstrate the presence of metallic gases in the atmosphere, by accurate chemical analysis, we shall receive it as a most interesting and unexpected fact; but, even then, we shall require much further evidence before admitting the existence of a "transparent metallic shell" enveloping the globe.

The following passage is likewise worthy of attention:—"We account for its [electricity] being a fluid, pervading everything, in perpetual motion, always trying to find its own level, but never doing so, by the fact that we compare it to the 'heart of minerals,' which, being constantly taken away from them, requires continually to be replaced; and when given off, and not replaced, causes the mineral to fall or crumble away into dust. Many have seen this in the case of iron when—lying in the form of anchors, guns, cables, &c.—it is exposed to atmospheric influences which produce rust. In time the whole ponderous mass is converted into oxide of iron, which, nevertheless, can be revivified when again subjected to heat. Whilst undergoing the process of re-melting it throws off its oxygen into the carbon, and re-imbibes that which it had originally lost and thrown out into the atmosphere." This "heart of minerals" is something with which we should like to be better acquainted. Can our author prove its existence, or show its properties and its composition? Some minerals, indeed, crumble to dust by the loss of water; but this is certainly not the case with iron, which, to the best of human knowledge, does not give off anything, but, on the contrary, receives something when it rusts away under the influence of the atmosphere. Electricity, according to the author, is not a mode of force, but a substantive entity which he describes as a fluid, gas, or body (call it what you like). The view that aërolites are formed within the earth's atmosphere Lieut. Armit thinks is "proved by their containing one-third of all the simple bodies known to exist in the earth!" This, we must beg to say, is no proof at all, unless it could be shown that these simple bodies were exclusively peculiar to the earth. That they have contained hitherto no matter foreign to our globe is also no argument in favour of their terrestrial origin. If the suns and planets have been formed, as we now consider, by the condensation of cosmic nebulæ, we must naturally expect that their elements will be alike.

One of the author's views—which if well founded is capable of direct experimental verification—is that the compass is affected, in a manner not as yet ascertained, by, or at least during, foggy weather. To this abnormal variation he attributes the recent loss of the *Schiller*, off the Scilly Islands. This is surely a matter which requires prompt and careful investigation.

We are far from pronouncing this work to be absolutely worthless. It contains much interesting and useful matter, but it requires a careful revision and the expurgation of certain crudities. What, for instance, must the reader think who reads that "India never lacks moisture," and is then told—on the very same page—that certain "vapour has gone to water Indian plains, where famine lately raged *owing to previous drought*"?

We hope to see the next edition freed from all the blemishes which we have felt it our duty to point out.

The Immortality of the Universe, considered in relation to the Persistence of its Motive Powers. By J. A. WILSON. Melbourne: G. Robertson. Auckland (N. Z.): Upton and Co.

THE author combats the hypothesis that the solar system, and indeed the whole universe, must come to an end, as far as its motions and activity are concerned by the dissipation of energy. The nature of his speculations may be perceived from the following summary which we quote:—

"The planetary motions are uniform, notwithstanding there are media in space impeding them. Hence the planets must be supplied with energy to enable them to perform their motions with regularity. To find this energy we endeavour to trace it to or from the sun, whose internal economy we do not, however, understand, and cannot interpret; notwithstanding we are able to discern objections to other interpretations. We have to consider the nature and action of radiance in regard to the molecules and atoms of substances. Referring to the solar power there is a fundamental objection to the 'law of the dissipation of energy;' as, if extended to the whole creation, the death of the universe would follow. Hence we submit a theory, namely, that—

"I. The sun receives back, in a latent form, the light and radiant heat expelled by him, and that gravity may be taken as the exponent of this return force.

"II. Or that the sun draws his supply from a common store, accessible to other suns, to which they each contribute. Here, also, gravity may indicate the power that collects and draws the nutritive element to the sun. We ought to take an extra-planetary view in our effort to conceive the connection of radiance with its source, nor should we permit the terrestrial phenomena of oxidation and combustion to distract our minds. The undulations of radiance possess motive power; they enter into substances, some of which—as steam and chlorophyll—become media capable of retaining and transporting them. Media that absorb the undulations in a potential, and emit them in an actual form, appear to have been provided in nature where their presence seems necessary; and nowhere in the universe does such necessity exhibit itself more strongly than where the vast majority of those undulations vibrate, where their power may be stored, and whence it may be transported and turned to account at the centres of activity. Such a medium may possess enormous space forces,—gravity which is inherent in matter being one of its modes, and forming a link between force and matter.

"The action of our ethereal medium, and the stream of radiance, may confer upon the planets their orbital and rotatory motion.

"The diurnal oscillation of the barometer is claimed as evidence of the external action of space forces upon our planet.

"This theory cannot explain the origin of the universe or the manner of the endowment of its motive powers; but it holds that an immortal universe is necessary as a habitat for immortal beings, and as a possession for its immortal Creator."

The Appendix on the Climatology of New Zealand is worthy of a careful examination.

The Creation: the Earth's Formation on Dynamical Principles, in accordance with the Mosaic Record and the Latest Scientific Discoveries. By ARCHIBALD TUCKER RITCHIE. London: Daldy, Isbister, and Co.

THE Bible has been put to strange uses. Placed in one scale of a balance, and weighed against a woman accused of witchcraft, it was sometimes made to decide the question of her guilt or innocence. Hung up from a key, or according to some *with* a key inserted between the leaves, it was expected to point out a thief amidst a circle of bystanders. Opened at random, in imitation of the *Sortes Virgilianæ*, it was supposed to afford an augury as to the success or failure of any undertaking. But it is very questionable whether any of these obsolete superstitions involves a more fundamental misconception of the rightful claims of the Scriptures than does the notion—still lingering in the British Islands, especially in the lowlands of Scotland and the north of Ireland,—of viewing them as a physical revelation, and of endeavouring to extract from them geological, chemical, or biological truths. Whenever, therefore, we meet with a so-called scientific treatise like the one before us, we know that the author has taken a radically false stand-point, and that we must prepare for a duty equally odious and unprofitable.

Mr. Ritchie is firmly convinced in his own mind of certain very strange propositions. He holds that the earth did not always rotate diurnally round its axis; that the sun, though the centre of gravity of the solar system, afforded no light; that the earth was a perfect sphere, covered everywhere "with a dark, unruffled mass of turgid waters;" and, above all, that there was no atmosphere! Under these circumstances, nevertheless, the waters were "the abode of innumerable races of living apulmonic creatures," of which there were several successive generations, whilst other organic beings were subsequently formed "during the six working days of the Mosaic week, each day consisting of twenty-four natural hours!" The author has unfortunately forgotten to add that in those dark, atmosphereless ages two and two made five, and two straight lines were capable of inclosing space. All this we are to accept "upon the authority of the immutable word of God, assisted by the discoveries of Science!" Readers are further told that "unless they are prepared to receive the announcements of Scripture"—as interpreted and applied, of course, by Mr. A. T. Ritchie—"with as implicit confidence as they would a thrice-demonstrated problem, it will avail them little to accompany" him. The author does not, as far as we have been able to trace, adduce any original observations or experiments in support of his views. A large part of his six hundred pages consists of quotations from scientific works, but these chiefly represent the state of knowledge of the earlier half of the nineteenth century. The "Cabinet Cyclopedia," the "Bridgewater Treatises," the "Library of Useful Knowledge," Dr. Fleming, Hugo Reid, and Nichol, seem his favourite authorities. There is, of course, in his work much that is true, and altogether beside the question. But to find truths peculiar and essential to the author's system is a difficult task. We quote, as most significant, the following passage from a note on p. 409:—"It is also worthy of remark how frequently throughout the Sacred Volume the finger seems to have been pointed to those discoveries long, long ago; and repeated at intervals, throughout the whole course of the Divine Revelation, with a clearness only equalled by man's wayward reluctance to appreciate them. In proof of this the following are a few from amongst the numerous passages which might be quoted to show that, under the figure of 'stretching out the heavens like a curtain,' the expansive principle, now termed the 'diffusion of gases,' is as clearly indicated as if volumes detailing the results of experimental philosophy had been written on the subject:—Ps. civ., 2; Isa. xl., 22; xliv., 24; xlv., 12; li., 13."

With this writer any serious discussion is impossible. That such a work can have been written, and have gone through several editions, is a painful proof of the backward state of higher education in our country. But we accept the author's motto—"Magna est veritas et prævalebit." We feel assured that before another century has elapsed this book and its theories will be remembered merely as a lamentable instance of wasted labour and misdirected ingenuity.

Gregory's British Metric Arithmetic, for the High School, the Board School, the Desk, and the Counter. By Isaac Gregory, F.R.G.S. London, Paris, and New York: Cassell, Petter, and Galpin. Manchester: John Heywood.

The author of this work undertakes to show the superiority of "British metric arithmetic (of sub-units and whole numbers) over French metric arithmetic of prime units and large decimals." He remarks, very justly, that sooner or later—and better sooner than later—the British people must adopt the standard of the metre, which has been accepted by the majority of foreign nations, and adopted by the outer commercial world, as the common basis of an international system of weights and measures. He has undertaken to solve the question how this great reform is to be effected, and proposes a modification of the French system, which seems singularly exempt from objectionable features. Thus, for professional use he would adopt the French weights, intercalating merely in the table 25 grms. = 1 oz. and 500 grms. = 1 lb. His table of commercial weights would be—

25 grms. = 1 oz.
20 ozs. 1 lb.
2 lbs. 1 kilo.
10 lbs. 1 stone.
10 stones 1 cwt.
20 cwts. 1 ton.

The kilo. of England being fixed the same as that of France, it is obvious that the new pound would be about 1½ ozs. heavier than the present standard, whilst the ounce would be a little lighter. The French system, as in actual use, is inconvenient in all those small transactions which make up the sum of retail traffic. There is no unit available for mental calculation between 1 grm. and 500 grms. (livre or demi-kilogrm.), or, again, between 1 grm. and 1000 grms. (kilogrm.). In the same manner, in French money there is no available unit between 1 centime and 100 centimes (1 franc), though English residents get over the difficulty by calling the 10 centime piece a penny. In proof that "giving change rapidly, say at a crowded railway-station, is a difficult matter with French money," he asks—"Where do you find in any country, except France or other centesimal moneyed countries, a humble though useful officer standing at a railway-ticket office, who is looked upon as an almost preternatural ready-reckoner, whose public function is to tell the intelligent clerk inside the window, and the passenger outside, how much change to give and receive out of a 5-franc piece for two tickets of 1 franc 85 centimes each. Twice 185 is beyond the reach of the multiplication table." Decimals, the author declares, are charmingly easy on paper, but not well adapted for mental calculation. He points out that "the mind, after finding the product by mental multiplication, which finishes the work in a system of integers, has to go in quest of the decimal points of the multipliers, reckon their conjoint value, and bring the conjoint decimal point down to the product, fix it correctly there between some two figures, and pronounce the money value of the product after this mental balancing." To do all this rapidly and correctly "in the head" requires a grasp of figures of which few minds are capable.

The French system is not, strictly speaking, "decimal:" in money it is centesimal, and in weights and measures millesimal. This serious difficulty Mr. Gregory proposes to avoid by the introduction of a metric ounce and pound, pint and gallon, and thus overcome the serious objection to the use of a decimal system.

We strongly recommend this work to our readers, who will find it much more interesting than the title might perhaps lead them to expect.

The High Antiquity of Iron and Steel. By St. JOHN V. DAY, Ass. Inst. C.E., F.R.S.E. London: W. H. Guest.

THE subject of this pamphlet and the results at which the author arrives may be understood from the following paragraph:—"There is no escape from the conclusion that in all the earliest peopled countries—whether peopled by Semitic, Hamitic, Aryan, or Allophyllian races—there is most certain proof that in the remotest ages which we can ascertain anything about the inhabitants were familiar with the use and manufacture of iron and steel; that in those countries there is not a tissue of evidence in favour of a bone or stone age, still less of a bronze and then of an iron age succeeding; that from the evidence adduced, and which indeed is being continually supplemented, it is evident the stone, bronze, and iron theory must be consigned to the limbo of false ideas and exploded notions." Do the author's researches do anything more than push back the period of stone implements, which, after all, are realities?

List of Elevations, principally in that portion of the United States West of the Mississippi River. By HENRY GANNETT. Washington: Government Printing Office.

THIS pamphlet gives the altitudes of a list of towns, mountains, passes, and table-lands in the western portion of the American Union, as also those of a few in foreign countries. We notice the wonderful elevation of New Mexico, Colorado, and Wyoming, in which the lowest town is situate at the extraordinary height of 4000 feet above the sea-level, whilst the highest —Montgomery, in Colorado—exceeds 11,000. The only error we perceive is in the height of Vesuvius, which, probably by a typographical error, is given as 8479 feet.

An Essay concerning Important Physical Features exhibited in the Valley of the Minnesota River, and upon their Signification. By G. K. Warren. Washington: Government Printing Office.

The author argues that Lake Winnipeg was at one time vastly larger than at present. At that period it discharged its waters, not as at present by the Nelson into Hudson's Bay, but by the Minnesota into the Mississippi; the Canadian lakes flowing, also, through Lake Michigan and the Illinois River, in the same direction. In support of this view he argues that the Nelson River and also the St. Lawrence bear, in their numerous rapids and falls, every mark of a comparatively recent origin. The Valley of the Minnesota, above its junction with the Mississippi, is out of all proportion too large for the former stream in its present condition, and only becomes intelligible if we consider it as the main artery of a vast tract of country. The Valley of the Illinois likewise appears disproportionate to the size of that river in modern times. These changes he accounts for by the assumption of a general subsidence of the land in the northern and eastern parts of the North American continent. By this change a new vent was opened for the waters of Lake Winnipeg and of the Canadian lakes, thus forming respectively the Nelson and the St. Lawrence, whilst the Minnesota and the Illinois, deprived of their main feeders, shrank into streams of an inferior rank.

The work is illustrated by several plans, and by a map showing the restoration of the ancient basin of the Mississippi. In this map the river is represented as rising in about lat. 56° N. and long. 40° W., draining the regions west of Hudson's Bay, passing through Lake Winnipeg, which then covered in all probability an area far exceeding that of Lake Superior, receiving the overflow of the Canadian chain through the Illinois, and falling finally into an estuary which penetrated to the influx of the Missouri.

The evidence in support of the author's theory has been very carefully collected, and is presented with great clearness.

Annual Report of the United States Geological and Geographical Survey of the Territories, being a Report of Progress of the Exploration for the Year 1873. By F. V. HAYDEN, United States Geologist. Washington: Government Printing Office.

THIS volume is especially devoted to Colorado, a region of great interest to the geologist. Amongst its valuable features we instance a catalogue of the minerals of Colorado, with their localities. Amongst these a prominent place belongs to certain tellurium compounds. These rare minerals occur at Red Cloud and Cold Spring, on both sides of a porphyry dyke, 50 feet in thickness, intersecting the granite, and striking about north-east. The tellurides of this region seem to show a greater variety of composition than those of any other locality hitherto known. Native tellurium occurs both massive and in small hexagonal crystals, with perfect lateral cleavage, forming columnar masses in white quartz. In hardness it ranges from 2 to 2·5. Its specific gravity is 5·802. It is lamellar in structure; tin-white to light steel-grey in colour; lustre splendent; streak sub-metallic, light grey to grey. It is less pure than the tellurium of Transylvania, containing only 90·85 per cent of tellurium, along with selenium, iron, and bismuth, and traces of gold and silver. Another telluride, occurring in the Red Cloud and Cold Spring mines, has received the name of Henryite. It is found in imperfect crystals, with good cubical cleavage, in thin threads, or in irregular foliated masses. Its hardness ranges from 2 to 2·5. Its specific gravity is 8·5253; its lustre metallic, splendent; colour bright silver-grey to steel-grey, but after exposure to the atmosphere for a short time it becomes a pale bright yellow. The streak is metallic, grey to silvery; it is opaque, brittle, partly malleable and sectile. Its composition is—

Lead	53·19
Iron	5·05
Silver	0·31
Gold	trace
Tellurium (by difference)	41·45
	100·00

which leads to the formula $3PbTe+FeTe$, a small portion of the lead being replaced by silver. In its physical characters this mineral approaches Altaite, which contains 60 per cent of lead, and no iron. Schirmerite, another new mineral, is found in thin threads and foliated masses. Its cleavage is perfect; hardness, 1 to 1·5; lustre metallic, splendent; streak dark grey to black; colour between light lead-grey and steel-blue; opaque, partly malleable and sectile; flexible and thin scales. It contains 18·82 per cent of gold, and 28·60 of silver. The residue comprises, besides tellurium, iron and a trace of lead. The formula is probably $3AgTe+(AuFe)Te$. The ratio of Au to Ag is 2 : 3, whilst in Petzite, the mineral nearest approaching to it, the proportion is 5 : 8. Petzite, moreover, contains on an average 25 per cent of gold and 40 of silver. The number of mineral species associated with these tellurides is strikingly smaller than is the case at Nagyag.

There are further special reports on palæontology, on zoology, and on geography and topography. The zoological department embraces Lieut. Carpenter's report on the collections made on the Survey in 1873; destruction of pine timber in the Rocky Mountains; report on the Alpine insect-fauna of Colorado; list of butterflies collected in Colorado; on the geographical distribution of moths in Colorado; report on the Diptera of Colorado; notice of the galls collected by Lieut. Carpenter; lists of the Coleoptera, Neuroptera, and Myriapoda collected by the same explorer; and a variety of other valuable papers. Among the Coleoptera figures, of course, *Doryphora decemlineata*, the redoubted Colorado potato-beetle, which a certain morning paper—prone to occasional and indiscreet dabblings in Science—describes as "a kind of cockchafer." It has never been found westward of the great water-shed, and appears to be travelling slowly, but steadily, eastwards. It does not occur at greater altitudes than 6000 feet.

The work is profusely illustrated with maps, plans, sections, and views of remarkable geological features.

Report of the United States Geological Survey of the Territories. By F. V. Hayden. Vol. VI. Washington: Government Printing Office.

It appears that whilst each State of the American Union is executing a geological survey within its own boundaries, the Federal Government is performing the same task in the so-called Territories—the regions not yet formally organised. These surveys are being conducted in the most thorough-going and elaborate manner, and their Reports will prove documents of high and enduring value to the geologists, palæontologists, zoologists, and botanists of the whole world.

The volume before us—merely a small instalment of the fruits of this gigantic undertaking—treats of the Cretaceous Flora of the Western Territories. It is illustrated with thirty plates, containing several hundred well-executed figures of leaves, stones, seeds, &c., of the fossil species described, and belonging to the Dakota group. Of this Flora the author remarks that, "without affinity with any preceding vegetable types, without relation to the Flora of the Lower Tertiary of our country, and with scarcely any forms referable to species known from coeval formations of Europe, it presents in its whole a remarkable, and as yet unexplained, case of isolation." He considers it probable that the first vegetable types, or at least the dicotyledonous ones, have appeared at the same or at different times, not only at different places, but with different original characters, constituting here and there distinct groups without homogeneity or relation of forms. Considering what is known of the succession of these groups, it seems as if some of the original types had persisted more or less indefinitely in the series, being modified perhaps by casual circumstances; and as if other original forms or prototypes had appeared here and there, and multiplied the characters of the vegetable groups." For a discussion on the origin of species, he considers the materials as yet too scanty. The flora of the cretaceous epoch alone, he remarks, comprises "many hundreds of thousands of species, comprised under more than fourteen thousand genera." If so—unless very different proportions prevailed between the various groups of the organic world from those which now obtain—the extinct insect-species of that epoch must have amounted to millions! But of these only units have left their traces in the "great stone book," which is in perhaps no other department so strikingly and so unfortunately imperfect.

A Manual of Electro-Metallurgy, including the Applications of the Art to Manufacturing Processes. By JAMES NAPIER, F.R.S.E., F.C.S., &c. Fifth Edition, revised and enlarged. London: C. Griffin and Co.

THIS work, in its present form, commences with a history of the galvano-plastic art, including an account of the earliest experiments of Spencer, Jacobi, and Jordan, with a list of works written on the subject, and a notice of the patents taken out for improvements in the process. From this the author passes to a description of galvanic batteries, electrotype processes, the method of bronzing, miscellaneous applications of the deposition of copper, the deposition of metals one upon another, electroplating and gilding, and the results of experiments on the deposition of other metals as coatings. The concluding portion of the work is devoted to theoretical considerations. The author, as he states in his preface to the fifth edition, upholds the unitary view of the electric force, and in electrolysis holds that the electricity is "conducted through the solution by the base or positive element in the electrolyte, which it does as if it were a solid chain of particles or wire." The statements made in the first edition concerning the practical operations of the art have not, we are told, required alteration.

A Dictionary of Musical Terms. Edited by JOHN STAINER, M.A., Mus. Doc., Magd. Coll., Oxford, and W. A. BARRETT, Mus. Bac., St. Mary Hall, Oxford. 456 pp. London: Novello Ewer, and Co.

THAT the subject of Music should be ably treated by the learned organist of our great metropolitan cathedral will not be a matter of surprise, but under the title of "A Dictionary" few will be prepared to find a volume which can be read with pleasure, in addition to being an extremely complete work of reference on musical matters. The Editors have done wisely in calling to their assistance authors capable of writing on special subjects; and the reader in search of precise information will be glad to meet with such articles as "Temperament," by R. H. M. Bosanquet, M.A.; "Licensing" and "Copyright," by J. Bulley, M.A., Barrister-at-Law; "Ear, Larynx, Laryngoscope, and Structure of the Hand," by F. Champneys, M.A., M.R.C.S., of St. Bartholomew's Hospital; "Acoustics," by A. E. Donkin, M.A., F.R.A.S.; in addition to many others on subjects especially musical.

Six pages are devoted to the article "Acoustics," and the subject is most ably treated in so small a space.

Under "Larynx" the structure of the vocal organs and their functions are most fully described,—not only in man, but comparative anatomy is entered into in considerable detail. The structure of the larynx is illustrated with no less than twenty-one woodcuts, showing the structure of the parts concerned in vocalisation and the various positions they take when in action. The researches of Garcia and others are given at great length, as also a very full account of the laryngoscope and the mode of making observations by its means. The structure and comparative anatomy of the ear are treated in an equally careful and elaborate manner.

Under the head of "Fingering," in addition to the strictly technical matter, chiefly of interest to the pianist, will be found an interesting account of the anatomy of the hand in especial relation to its use on the fingerboard of keyed instruments.

The work is one of great value to lovers of music; almost all terms, whether English or foreign, are to be found in it, and a vast amount of information which can only have been the result of most indefatigable research on the part of the various editors. It is printed in the usual handsome manner of the publications of Messrs. Novello.

A Short History of Natural Science. By ARABELLA B. BUCKLEY, London: John Murray.

To furnish, within the compass of five hundred pages, a history of natural science, and of the progress of discovery from the time of the Greeks down to the present day, may well be considered a bold undertaking, even though the work is primarily intended for the use of schools and of young persons. The authoress seeks "to place before young and unscientific people those main discoveries of science which ought to be—but which are not—known by every educated person, and at the same time to impart a living interest to the whole by associating with each step in advance some history of the men who made it." She adds—"I have often felt very forcibly how many important facts and generalisations which are of great value in giving a true estimate of life and its conditions are totally unknown to the majority of otherwise well-educated persons." This is true, and "pity 't is 't is true."

Upon the whole we may fairly congratulate Miss Buckley upon the success of her labour, and must declare that she fulfils, and often more than fulfils, her promises. She is not content to give a mere enumeration of discoveries in their chronological order. A number of phenomena and of inductions—astronomical, physical, chemical, and biological—are explained in clear and simple language, and are illustrated with diagrams. Thus a very small amount of preliminary knowledge is required from the reader, and no one will find himself repelled by a parade of needless technicalities. Of course, keeping within such narrow compass, and addressing herself not to the learned but to learners, the authoress cannot be expected to enter into minute details, or to give an exhaustive and authoritative judgment on the merits and claims of rival discoverers; but she furnishes a broad and mainly correct outline, which the thoughtful reader will doubtless seek to fill up.

The arrangement of the work is very simple. The first part describes the state of Science among the Greeks; the second treats of the Science of the Middle Ages, from A.D. 700 to A.D. 1500; and the third, and of course largest section, carries the reader down to the present day; each of the two former parts, and in the modern division the account of each century, being preceded by a list of its chief men of science, with the dates when they flourished. Every chapter concludes with a list of the principal authorities consulted. As must be expected in a work of this nature, there are passages on which a variety of opinions may prevail; but in dealing with all controverted questions the authoress shows great moderation, and keeps within the truth. This is especially shown in her manner of dealing with the martyrs of science, and with the Darwinian controversy. In expounding the great doctrine of organic evolution she shows that it has no more connection with atheism than has the old and opposite view of the independent origin of species. The following passage is worthy the careful attention of all, however eminent, who have allowed prejudices to blind their eyes to the truth:—

"It is extremely foolish to be prejudiced against it, as some people are, by the idea that animals formed in this way can be less God's creation than if they were made in any other way. The whole history of Science teaches us that men, in all ages, have constantly taken false alarm when it has been shown that God's ways are not our ways, and that the universe is governed by far wider and more constant laws than we had imagined in our little minds. But in the same way as the planets are none the less held in God's hand because we now know that it is by the law of gravitation that he governs their movements, so every plant and animal must be equally His creation, in whatever way they have been developed."

Surely this passage, equally reverent and philosophical, is a noble rebuke to all the frantic denunciation of Evolutionism which has been poured not merely from the pulpit and the political press, but which has also, *proh pudor!* been vented from the professor's desk and expressed in learned journals.

To one oversight we beg to draw Miss Buckley's attention:—The Roman general who was besieging Syracuse in the days of Archimedes was not Mæcenas, but Marcellus.

We can strongly recommend this book, not merely to the young, but to many persons of more mature age. It will, we think, give them a sounder, worthier conception of the achievements, the promises, and the claims of Science.

Proceedings of the Literary and Philosophical Society of Liverpool, during the Sixty-fourth Session, 1874-5. No. XXIX. London: Longmans and Co. Liverpool: D. Marples and Co. (Limited).

The gale which was stirred up by the ever-memorable "Belfast Address" of Dr. Tyndall seems not yet to have subsided, and is blowing very briskly in the pages of the volume before us. Three at least of the papers herein-contained—to wit, an address "On the Materialism of Modern Science," by A. J. Mott, President; an essay on "Potency in Matter," by the Rev. H. H. Higgins; and Part III. of a somewhat *buffo* memoir on "Philosophy without Assumptions," by T. P. Kirkman, F.R.S.—appear to be aimed, more or less directly, at Dr. Tyndall, Prof. Huxley, Mr. Darwin, Mr. Herbert Spencer, Mr. Justice Grove, and those mysterious bugbears the "Materialists." It was once the rule that scientific societies, metropolitan or provincial, should eschew metaphysics and theology, for the discussion of which there is surely ample room elsewhere; but at Liverpool it seems this prudent reservation, which after all is only a case of the division of labour, is thrown aside.

The very opening sentence of the President's Address has a strange ring about it:—"The time is near at hand, if we may judge our age by its tendencies, when the pursuit of Science will have to justify itself anew to the reason of mankind." To the reason of mankind it requires no justification. Whether it ever will succeed in justifying itself to their prejudices and their stupidity is very doubtful. That happiness does not increase "in the world in the ratio of our intellectual acquirements" may be very true—but wherefore? Because the increase of knowledge has been simultaneous with other changes with which it has no necessary connection. For it is not the increase of our intellectual acquirements, or our intenser and more general devotion to scientific research, which narrows the margin between each man and want, and which makes the career of the vast majority a mere frantic struggle for existence. If the lives of learned men are "melancholy," is it not to some extent due to the manner in which they are treated by their fellows? Health may be injured in the workshop and the counting-house as well as "in the laboratory;" eyesight may be "dimmed" by poring over account-books as well as by gazing at the glories of the heavens; "time which never returns" may be spent on the Stock-Exchange as decidedly as in the "severities of study." Over-work, the bane of our age, is in no wise peculiar to men of science, and is in their case accompanied with far less anxiety than among men of business. Nor is it safe to assume that Science may with impunity be neglected by a nation because some men devote themselves to study by an irresistible impulse. We must not forget that the country which gives the greatest encouragement to discoverers will infallibly take the lead, alike in the arts of war and of peace. Into the remainder of this address we cannot enter.

Mr. H. H. Higgins takes up, again, the same subject. We can fully agree with him in his protest against the doctrine, recently revived, that animals are automata. Such a view might be pardonable in Descartes and Leibnitz, if a man may be pardoned for dogmatising on subjects which he has not made his especial study; but it is intolerable in Prof. Huxley, except he is prepared to prove that he himself is an automaton. We cannot, however, blame him for insisting that every proposed theory should be allowed to stand or fall upon its own evidence. What court of justice, in deciding upon the guilt or innocence of an accused person, takes into account, as a guiding element, the effect which such decision may have upon the welfare of others?

Concerning the doctrine of Evolution the author asks—"How is it there should be such difficulties, and whether their existence is not a significant fact?" Difficulties do not, so far as I know, beset in any like degree theories relating to things without life. "Theories in Astronomy, Chemistry, Light, Heat, Magnetism, &c., work smoothly enough." The answer to these questions lies in the far greater complexity and difficulty of the organic sciences, and in the comparatively short time that we have had on these subjects anything worth calling a theory. In the inorganic sciences we have had to modify our theories, and have, in some departments, arrived at a close approximation to the truth. As Prof. Huxley aptly pointed out, it was at one time supposed that the planets revolved round the sun in orbits perfectly circular. This view explained certain facts, but there remained discrepancies, difficulties. The theory did not work smoothly. The orbits were then declared to be elliptical, and the discrepancies faded away. Is it unreasonable to expect that our zoological and botanical theories may be in like manner gradually improved ?

So far we have been listening to men who at least were disposed to treat their subject seriously. But Mr. T. P. Kirkman is a writer of a different order : his object, apparently, is to make modern physicists and physiologists ridiculous to outsiders, and to demand for " parsons, poets, metaphysicians, and moralists " the liberty of laying down the law on matters which they have never studied. He jests and quibbles, and parodies and travesties the names of his opponents, pats them at times on the head in the style of a condescending schoolmaster, and praises a work of his own—doubtless with the kind intention of saving " trade-reviewers " the trouble. To criticise an author who is on such very excellent terms with himself would be labour lost. So we leave Mr. Kirkman, his " will," and his " philosophy without assumptions," which cannot be considered an unassuming philosophy.

From these arid regions it is a positive relief to turn to a paper on " Sponges, their Anatomy, Physiology, and Classification," by Mr. Thomas Higgin, which shows that some, at least, of the members of the Society are willing and able to produce valuable scientific work.

Descriptive Catalogue of the Photographs of the United States Geological Survey. By W. H. JACKSON. Photographer. Washington : Government Printing Office.

IF there still exists a man who entertains doubts concerning the splendid services which photography can render to all branches of natural science, let him look at the specimens accompanying this catalogue, and henceforth hold his peace. " By no other means," as the author with perfect truth declares, " could the characteristics and wonderful peculiarities of the hitherto almost unknown western half of our continent be brought so vividly to the attention of the world." No amount of description would tell the geologist one-half of what he can learn concerning the rock-formations of the regions illustrated by a mere glance at these wonders of art. Nor could any draughtsman, however skilful and conscientious, accomplish, in any reasonable amount of time, the work here done by the camera in an instant. We need only cite, as instances in point, the view of "Cathedral Spires, Garden of the Gods," or of the "Eroded Sandstones of Monument Park." Perhaps the most wonderful production is the "Mountain of the Holy Cross." Such a happy rendering of a mountain tempest we should scarcely have imagined as possible. Through dense storm-clouds and rain-torrents a broad gleam of sunlight streams upon the brow of the mountain, and upon the mists boiling up in the ravines below. The view of the Upper Twin Lake, Colorado, if of less geological interest, is not less wonderful as a work of art. The reflections of mountains and trees in the lake, the evening sky, and the smoke-wreaths ascending from the camp-fire of the Explorers, are truly wonderful.

Nor are geology and physical geography the only branches of science upon which photography is thus shedding a new light. The archæology of pre-historic races, the ethnography of tribes still existing, and not less the zoology and botany of sparingly explored regions, may all share in the benefit.

We cannot better express our high opinion of these illustrations than by wishing that every scientific expedition, in what part of the world soever, may include among its staff a photographer as skilful as Mr. W. H. Jackson.

PROGRESS IN SCIENCE.

MINERALOGY.—Deposits of crude borate of soda have been found by Mr. Robottom, of Birmingham, at the bed of a dry lake in the Slate Range Mountains. We learn, from the "California Alta," that the appearance of the surrounding country clearly indicates that water once stood 60 feet deep here over a large area, the ancient beach being distinctly traceable. The most remarkable fact about this saline product is, that in its middle there is a tract 5 miles long and 2 wide of common salt, while on the outside there is a deposit of borate of soda 3 feet thick, and under this a lower stratum composed of sulphate of soda and tincal mixed together from 1 to 3 feet thick. The borate of soda is easily crystallised, and on exposure of the crystals to the sun after they are taken out of the vat a white powder is obtained, which is preferred by some of the potters to the refined borax of the English market. For cleansing purposes this borate of soda is far more economical, and possesses great advantages over common soda and washing-powders.

At a meeting held at the Scientific Club, on the 3rd of February, it was resolved to establish a Society to advance the study of Mineralogy and Petrology. The name of the new Society is the Mineralogical Society of Great Britain and Ireland. The following officers have been elected:—President—H. C. Sorby, F.R.S., &c. Vice-Presidents—The Rev. Prof. Haughton, F.R.S., &c.; Prof. M. Forster Heddle, M.D., &c. Council—D. T. Ansted, F.R.S., &c.; The Rev. T. G. Bonney, M.A., &c.; Prof. Church, M.A., &c.; W. Crookes, F.R.S., &c.; Fred. Drew, F.G.S., &c.; Major Duncan, M.A., D.C.L., &c.; Archibald Geikie, F.R.S., &c.; Capt. Marshall Hall, F.G.S., &c.; Prof. T. Rupert Jones, F.R.S., &c.; Prof. Jas. Nicol, F.R.S.E., &c.; Prof. F. W. Rudler, F.G.S., &c.; R. H. Scott, M.A., F.R.S., &c. Trustees—The Right Hon. Lord De Blaquiere, F.R.G.S.; Patrick Dudgeon, F.R.S.E. Treasurer—R. P. Greg, F.G.S., &c. General Secretary—J. H. Collins, F.G.S. Foreign Secretary—C. Le Neve Foster, B.A., D.Sc., &c. Auditors for 1876—M. Hawkins Johnson, F.G.S.; B. Kitto, F.G.S.

It is proposed by Prof. E. T. Cox to distinguish, under the name of *Indianite*, a pure white clay which forms an extensive deposit in Lawrence Co., Indiana. The clay greatly resembles kaolin, and is worked for use in the porcelain factories of Cincinnati.

Under the term *Achrematite* Prof. Mallet, of Virginia, has described a new molybdo-arsenate of lead, occurring at Guanaceré, in Mexico.

Siegburgite is the name which Dr. A. von Lasaulx has applied to a new fossil resin found in the sands overlying the lignites of Siegburg, on the Rhine. It appears that the resin is found in various stages of oxidation, and its composition is therefore not constant.

In compliment to the great Italian geologist, Prof. Gastaldi, the name of *Gastaldite* has been bestowed upon a new mineral, by Prof. Strüver, of Turin. It is a silicate of alumina, soda, and protoxide of iron, crystallising in the monoclinic system, and occurring in the deposits of copper ore at Champ de Praz and S. Marcello, in the Val d'Aosta.

The Cornish mineral described a few years ago by Prof. Maskelyne, under the name of *Andrewsite*, has been analysed by Dr. Flight, and from this analysis the follwoing formula may be deduced:—

$$2(2Fe_2P_2O_8+Fe_2H_2O_4)+CuH_2O_2.$$

Dr. C. Le Neve Foster has read before the Geological Society of Cornwall some notes "On the Place and Mode of Occurrence of the Mineral Andrewsite." It appears from this paper that the mineral in question occurs in a tin lode coursing through granite, at West Phœnix Mine, near Liskeard.

Associated with some of the Andrewsite is a mineral which Prof. Maskelyne has referred to Ullmann's *Chalkosiderite*. Some crystals from West Phœnix have been measured, though with difficulty, by Prof. Maskelyne, who refers them to the Anorthic System, and publishes in the "Journal of the Chemical Society" a projection of the faces and a description of the crystals.

Attention has been called by Mr. Stoddart to the occurrence of gold and silver in carboniferous limestone, at Walton, in the neighbourhood of Clifton, near Bristol.

Dr. Hayes has submitted to the American Academy of Sciences a paper in which he traces the wide distribution of compounds containing phosphorus and vanadium through a great number of sedimentary rocks. Herr Hilger has lately determined the presence of lithium in a great number of sedimentary rocks.

To the "Journal of the Chemical Society" Mr. J. A. Phillips contributes a paper descriptive of the well-known pseudomorphic crystals which were found some years ago at Wheal Coates, near St. Agnes, in Cornwall. These crystals are pseudomorphs after twins of orthoclase, and are found, on microscopic examination, to consist, in some cases, of micaceous plates, particles of quartz, and crystals of cassiterite; whilst in others the cassiterite predominates, and is associated with blue tourmaline or indicolite. Analyses of both varieties are published by Mr. Phillips.

Mr. R. Pumpelly has described some pseudomorphs of chlorite after garnet, which occur abundantly in a bed of chloritic schist, overlying magnetite, in the Huronian Series, at Spurr Mountain Iron-mine, Lake Superior.

Some interesting crystals of Chondrodite, from the Tilley Foster Iron-mine, Brewster, New York, have been described by Mr. E. S. Dana. The mode of occurrence of the chondrodite at this locality has been described by Prof. Dana, but his son now publishes a memoir specially devoted to the crystallographic description of the mineral. The great interest of chondrodite lies in its relation to the Vesuvian humite, the two minerals being identical in chemical composition and exhibiting closely related crystalline forms. Three distinct types of humite have been recognised by Scacchi and Vom Rath, and Mr. Dana is enabled to show that all three types are paralleled in chondrodite. It is interesting to mark the influence of the Vienna school of crystallography on Mr. Dana, his memoirs—whether in Tschermak's "Mittheilungen" or in his own Journal—being illustrated by Miller's symbols, and with stereographic projections of the poles of the observed faces.

A zeolitic mineral, occurring at Neepigon Bay, on the north shore of Lake Superior, was described some time ago by Prof. Foote, under the name of *Zonochlorite*. Mr. Hawes has since analysed this substance, and finds it to be merely an impure variety of prehnite. In its mode of occurrence zonochlorite resembles the well-known *chlorastrolite*, which is found in amygdaloidal trap and in derived pebbles on Isle Royale, Lake Superior. After careful microscopic examination of chlorastrolite, Mr. Hawes concludes that it is not a homogeneous body, and that a large proportion of the stone consists of impure chlorite.

An analysis of a gypsum from White Mountain, in Southern Utah, has been contributed to the "Chemical News" by Dr. Machattie. The gypsum contains a notable proportion of carbonate of lime. An analysis of Peruvian *caliche*, or soda-nitre, is published by the same chemist.

The second Appendix to Prof. Dana's valuable "System of Mineralogy," prepared by Mr. E. S. Dana, has just been published, and brings the work up to January 1875.

GEOLOGY.—At the Annual General Meeting of the Geological Society, held on the 18th of February, the Wollaston Medal was presented to Prof. Huxley, in recognition of his distinguished services to geological science. The balance of the proceeds of the Wollaston Donation Fund was handed to Mr. J. Gwyn Jeffreys, for transmission to Prof. Guiseppe Seguenza, F.C.G.S., of Messina, for his investigations upon the Tertiary beds of Italy and Sicily. The Murchison Medal was handed to Prof. Ramsay, for transmission to Mr. A. R. C. Selwyn, F.R.S., F.G.S., in recognition of his services to Silurian geology; the balance of the Murchison Geological Fund being also handed to Prof. Ramsay, for transmission to Mr. James Croll, in recognition of his valuable past labours, and in the hope that his enquiries would be still further prosecuted. The first Lyell Medal and the entire proceeds of the Fund were presented to Prof. Morris. The President prefaced his Anniversary Address by some obituary notices of Fellows and Foreign Members deceased during the past year, including Sir Charles Lyell, Mr. Poulett Scrope, Sir Wm. Logan, M. G. P. Deshayes, Mr. W. J. Henwood, Mr. W. Sanders, Archdeacon Hony, Sir Edward Ryan, and others.

In the "Quarterly Journal of the Geological Society," published in February last, is a paper by Mr. Thomas Belt, in which he describes the drift of Devon and Cornwall, and correlates it with that of the South-east of England. He divides the beds into two series, namely, Upland and Lowland deposits, and discusses their relation and origin. He shows that the gravels and transported boulders require the presence of water up to the highest level at which they are found, or about 1200 feet above the sea. He considers that this water was that of a great lake, formed by the bed of the Atlantic Ocean being filled with ice down to about lat. 39° on the American side and at 49° on the European side. This ice blocked up the English Channel, and with it all the drainage of Northern Europe, which was pounded back, and formed a great lake over which icebergs floated. The breaking away of the icy barrier caused the sudden and tumultuous discharge of the great lake, and the outspread of the Lowland gravels over the lower grounds. The author cites the known facts respecting the glaciation of Iceland, the Shetland Isles, the Hebrides, the North of Scotland, and the West of Ireland, as evidence in favour of his theory that ice did flow down the bed of the Atlantic from the direction of Greenland. He also argues that palæolithic man, the mammoth, and other large mammals, lived before the glaciation of the country, and not afterwards. They occupied, he thinks, the banks of a large river and its tributaries that flowed to the south-west, through what are now the Straits of Dover, before the advance of the Great Atlantic glacier. The theory of the pre-glacial age of palæolithic man, and also that of the bed of the Atlantic, having been occupied by ice, were brought forward by the author, in his paper on "Niagara," published in this Journal, in April, 1875.

Mr. J. Clifton Ward has recently communicated to the Geological Society the results of his investigation of the points of theoretic importance connected with the Granitic, Granitoid, and Associated Metamorphic Rocks of the Lake District. The paper is divided into five parts, the first four relating to the origin of the Plutonic rocks of the district, and the degree of alteration to which the surrounding rocks have been subjected. In Part I. the evidence of the liquid-cavities in the quartz of the granitic and granitoid rocks was considered, the general conclusion being that the granites, syenitic granites, and quartz felsites were all consolidated at very considerable depths, under great pressure, this pressure being much greater than could be due to the thickness of overlying rocks, and therefore exerted mainly from below and laterally, and resulting in the work of upheaval and contortion of the overlying strata. The period at which the principal formation of these granitic and granitoid rocks took place was considered to be that of the Old Red; and the work of elevation, consequent on the great surplus of upward and lateral pressure, was accompanied by an enormous denudation of rocks at the surface during the greater part of Old Red times. In the subsequent divisions of this memoir the mode of origin of these various masses was discussed. In Part II. the granites of Eskdale and Shap were dealt with, and it was shown to be at least probable, from evidence gathered in the field, and by microscopic and chemical examination, that these granites had been formed by the extreme metamorphism of rocks of the volcanic series, while at the same time the *partially* intrusive character of the Shap granite was suggested by various considerations.

Part III. discussed the origin of the Skiddaw granite from points of view furnished by field, microscopic, and chemical investigation. The gradual transition from unaltered Skiddaw slate to mica-schist was proved under these three heads, while at the same time the abrupt passage from the mica-schist to the granite appeared to negative the idea of the next step, into granite, necessarily following in this case. In Part IV. the quartz felsite of St. John's and the syenitic granite of Buttermere and Ennerdale were examined as to their origin; and there was found to be much evidence in favour of their representing transition beds between the volcanic series and Skiddaw slates, metamorphosed *in situ*. The interesting rocks of Carrock Fell were then considered, and field, microscopic, and chemical evidence were all thought to lead to the inference that these masses of felsitic, dioritic (?), and hypersthenitic rocks were due to the metamorphism *in situ* of the beds forming the lower part of the volcanic series. In Part V. the author points out several considerations relating to metamorphism, to which the geological facts of this district seem to lead. It would, he says, be "unwise to suppose that every granitic mass has been the root and origin of some past series of volcanic phenomena; for it may represent (1) a mass so deeply formed that, notwithstanding all the elevation and contortion produced by the intense pressure, no point of sufficient weakness was found whereby relief might be obtained and a volcanic centre be established; (2) a mass which, though very deeply formed, was yet able to penetrate upwards for a certain distance along some area or line of weakness, though its final point of consolidation was still far below the surface, with which it for ever remained unconnected by any volcanic neck; or (3) it may actually represent the very foundation of a true volcanic neck." The author also treats of what he terms "selective metamorphism," and concludes his memoir by summarising the principal conditions under which rocks occur with regard to metamorphism:—(1.) That state in which igneous fusion is the most important or conspicuous element. (2) A state of aqueo-igneous fusion, occurring at a much greater depth than the last, and reaching only a dull red-heat as a maximum. (3.) A state in which the rocks are permeated by water at a considerably lower temperature than 400° C.

A useful, descriptive, Catalogue of Rocks, Minerals, and Fossils, illustrative of the Geology, Mineralogy, and Mining Resources of Victoria, has been prepared by Mr. R. Brough Smyth, F.G.S., F.L.S., Secretary for Mines and Chief Inspector of Mines for the Colony.

We have received No. 6 of the Second Series of the "Bulletin of the United States Geological and Geographical Survey of the Territories," by Mr. F. V. Hayden, U.S. Geologist in charge. The Bulletin of the Survey was commenced in 1874, for the purpose of giving to the world, more rapidly than through the Annual Reports, the vast amount of new material which was constantly accumulating under the auspices of the Survey. The success attending the publication was so great that it commenced the year 1875 as a regular serial, and six numbers have been issued of about five hundred closely-printed octavo pages, with twenty-six pages of maps, sections, and other illustrations, with table of contents and full index.

MICROSCOPY.—A new form of single magnifier has recently been contrived by Mr. John Browning. It is thus described by the inventor:—"The platyscopic lens is a triple achromatic combination, in which the chromatic and spherical aberrations are corrected by a central lens of dense glass. This lens is nearly three times as thick as the crown-glass lenses. The interior curves are almost hemispheres. The final correction for spherical aberration is made by altering the thickness of the dense glass lens. The three lenses are united by a transparent cement which has a refractive index corresponding very nearly with that of glass. This prevents light being lost by reflection from the surface of the deep curves. The platyscopic lens is made of three degrees of power, magnifying respectively 15, 20, and 30 diameters." Microscopists who have occasionally employed a low-power achromatic objective as a hand magnifier cannot fail to have been struck with its brilliant definition: indeed the only drawbacks to its use have been its awkward form of mount and its great cost. The platyscopic lens admits of the same freedom in the manner of holding which makes the Coddington so convenient, while its freedom from chromatic and spherical aberration renders its definition vastly superior. Its working distance is also much greater than that of any lens of equal magnifying power: this is very perceptible in the smallest of the series, which gives so much space that opaque objects can be viewed with the greatest facility—an important matter to naturalists in field observations. The spines on the pollen of the Malvaceæ are easily seen, and a very good view is obtained through a considerable depth of water. The performance of the lowest power is wonderful for so small an amplification, and, from its large field and long working distance, makes a good lens for dissection or other minute operations, especially when mounted on a suitable stand. Owing to the perfect corrections of the platyscopic lens, the internal diaphragm made in the Coddington by grinding out the equatorial portion is not needed: the result is a vast increase in illuminating power, enabling the glass to work well in very bad lights. It is the most perfect single magnifier yet produced.

The address of H. C. Sorby, F.R.S., President of the Royal Microscopical Society (Trans. Roy. Micr. Soc., Feb. 2, 1876) contains a vast amount of interesting matter. The limit of the power of the microscope is discussed at some length, and especial attention drawn to the difficulty of distinguishing true structure from interference fringes when the intervals between the real markings are of the same order of magnitude as half the length of the waves of light. This effect is altogether independent of the quality of lenses. It depends on the physical constitution of light itself, and would only be the more perfectly seen with more perfect object-glasses. Some very intricate calculations are entered into with regard to the size of the ultimate atoms of matter. This part of the subject is too complicated to admit of an abstract. A careful perusal of the paper will repay those interested in so important a subject.

The subject of Micro-photography has been brought before the Medical Microscopical Society by Mr. G. M. Giles. The principal novelty is the way in which the employment of the heliostat is dispensed with. This is effected by the use of a long focus condenser, giving an image of the sun of such dimensions that it remains in field a sufficient time to permit of the object being photographed. The lens employed is an achromatic, photographic single combination, of 3½ inches diameter, and about 10 inches focal length. The microscope is inserted into a "bellows" camera, capable of being drawn out to a length of about 2 feet, and of a size to take plates 6 inches square. The mirror is about 4½ inches square, is mounted so that its movements may be directed by means of cords running over grooved pulley: this is needed on account of the great length of the apparatus. The remainder of the fittings are of a kind that would suggest themselves to any practical photographer.

Dr. M. H. Stiles, in an account of a method of staining and mounting wood sections, calls attention to a few points of manipulation which may prove of interest to microscopists. For softening dried specimens for cutting, a mixture of equal volumes of alcohol, glycerin, and water is recommended. For bleaching previously to staining, if required:—A solution of ¼ ounce of chloride of lime in a pint of water, shaking occasionally for an hour, and, after allowing the sediment to subside, decanting the clear solution. After pouring off the bleaching solution, wash the sections by soaking them for at least twelve hours in water, changing frequently, and finishing off with distilled or rain water. The elimination of the chlorine will be much facilitated by placing the sections, after removal from the bleaching liquid, in a solution of hyposulphate of soda (1 drachm to 4 ounces of water) for an hour, and then washing as directed. Dr. Stiles, after staining, bleaching, and washing the sections, washes them three or four times with spirit, and, after draining, soaks them in oil of cajeput * for an hour: at the end of this time the oil is removed, the sections drained on blotting-paper and immersed in turpentine: they are then ready for mounting in balsam or damar. Oil of cajeput is preferred to oil of cloves as being more limpid, much cheaper, and not giving its own colour to the tissue: it is freely miscible with spirit and turpentine, in all proportions.

ELECTRICITY.—Dr. Kerr, of Glasgow, has discovered a new relation between electricity and light. He finds that when plate glass is intensely dielectrified, and traversed by polarised light in a direction perpendicular to the lines of force, it exerts a partially depolarising action upon the light, giving an effect which is much more than merely sensible in a common polariscope. There is a good regular effect when the plane of polarisation is at 45° to the lines of force; no regular effect when the plane of polarisation is parallel or perpendicular to the lines of force. Electric force and optical effect increase together. The optical effect of a constant electric action takes a certain time to reach its full intensity, which it does by continuous increase from zero; and it falls again slowly to zero after the electric force has vanished. The dielectrisation of plate glass is equivalent optically to a compression of the glass along the lines of electric force. Dielectrified glass acts upon transmitted light as a negative uniaxal with its axis parallel to the lines of force. Dielectrified quartz, like glass, acts upon transmitted light as if compressed along the lines of force, while dielectrified resin, unlike glass, acts as if extended along the lines of force. Carbon disulphide is birefringent when dielectrified, acting upon transmitted light as glass extended along the lines of force. The electrostatic force and the birefringent power increase together; they also vanish simultaneously, the optical effect disappearing abruptly and totally at the instant of electric discharge. Of the liquids examined there are six which have given definite and constant results, namely, carbon disulphide, benzol, paraffin and kerosene oils, oil of turpentine, and olive oil. These bodies are distinctly birefringent when dielectrified, acting upon transmitted light as uniaxal crystals with axes directed along the lines of force, the uniaxal being negative in the case of olive-oil, positive in the other five cases. Dielectrified olive-oil acts in the same way as dielectrified glass, or as glass compressed along the lines of force; the other five liquids dielectrified act as resin dielectrified, or as glass extended along the lines of force. Compared among themselves, with reference to strength of birefringent action, the liquids appear to be very unequal—carbon disulphide the strongest, paraffin and kerosene the weakest. Compared with glass they are much weaker insulators; but if allowance be made for this difference, for intensity as well as purity of effects, carbon disulphide is far superior to glass. In contrast with glass, all the liquids are characterised by the absence of coercive force, and by the rapidity of variation of birefringent action from point to point of the electric field. The birefringent power is sustained in liquids by the present action of electric force at each instant: it seems also to be determined at each point, simply by direction and intensity of force at the point. The author enunciates the three following assumptions:—(1.) The particles of dielectrified bodies tend to arrange themselves in files along the lines of force. (2.) Changes of molecular arrangement, consequent upon rise or fall of electric action, are effected slowly and with difficulty in solids, easily and at once in liquids. (3.) The lines of electric force, or the axes of molecular files, are lines of compression in one class of dielectrics (glass, &c.), and lines of extension in another class (carbon disulphide, &c.). The facts, when thus interpreted, afford a strong confirmation of Faraday's theory of electrostatic induction; and in whatever way interpreted they give promise of some new insight into that interesting subject, the molecular mechanism of electric action.

TECHNOLOGY.—In his "Report on the Development of the Chemical Arts during the last Ten Years," Dr. Hofmann says that decolorisation is effected

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* Distilled from the leaves of *Melaleuca minor*, a plant growing in the Molucca Islands.

less rapidly by peroxide of hydrogen than by chlorine. Tessié du Motay and Maréchal mention it as one of the agents which they propose for bleaching tissues, which, after treatment with permanganate of potash, they recommend to be steeped in a solution of peroxide of hydrogen. But it had been much earlier applied as a bleaching-agent by Thénard himself for a particular purpose—namely, for restoring old oil-paintings and drawings. White-lead in old paintings, which has become blackened by the gradual action of sulphuretted hydrogen, is converted into sulphate of lead by dilute solutions of peroxide of hydrogen, and thus restored to its primitive colour. A fine drawing by Raffaelle, with superimposed white which had become spotted with black, was completely cleansed by a solution which contained at most five or six times its volume of available oxygen, and the paper did not suffer. A peculiar application of this bleaching-agent has been made public by A. v. Schrötter and others. During the last few years bottles labelled "Eau de Fontaine de Jouvence, golden," and containing about 140 c.c. of a colourless liquid, have been sold by perfumers in great cities: to them, it appears, is due that offensive blonde shade of hair which holds an intermediate place between ash-grey and bright yellow, and attracts the attention of the spectators and the curiosity of observers by its *piquante* unnaturalness. This secret nostrum is merely a solution of hydrogen peroxide made stable by copious dilution, and by addition of a small quantity of acid—apparently nitric acid. A bottle costing 20 francs yields the purchaser 2·5 to 4 grms. of this substance in solution, and effects its purpose completely, though slowly, within four to six days, thus strikingly illustrating the great efficacy of peroxide of hydrogen. This would not be the first body whose industrial application commenced with trifles and gradually reached an unimagined extension. Nitrate of silver served first the vanity of the world as a hair dye long before its applications in photography. Schrötter very rightly expresses the wish that peroxide of hydrogen might be generally accessible at a moderate price. That for medicinal purposes it is preferable to oxygen and ozone is manifest. Whilst ozone only bleaches ivory in the strongest sunshine of summer, there is no doubt but that peroxide of hydrogen would answer the same purpose even in the absence of light.

Mr. Thomas Routledge, of Sunderland, who in 1860 was the only paper-manufacturer using esparto, the supply of which is now decreasing, has called the attention of paper-manufacturers to the probable advantages that would be derived from the employment of bamboo as a cheap and useful paper-making material.

Up to 1840 mirrors were silvered exclusively by means of an amalgam of tin—a process most destructive to the workmen employed. An important step was effected by an English chemist, Drayton, who conceived the idea of coating mirrors with a thin layer of silver, obtained by reducing an ammoniacal solution of nitrate of silver by means of highly oxidisable essential oils. This process was subsequently modified by several chemists, but only became really practical when M. Petitjean substituted tartaric acid for the reducing agents formerly employed. The glass to be silvered is laid upon a horizontal cast-iron table heated to 40°. The surface is well cleaned, and solutions of silver and of tartaric acid, suitably diluted, are poured upon it. The liquid, in consequence of a well-known effect of capillarity, does not flow over the edges, forming a layer of some m.m. in thickness. In twenty minutes the silver begins to be deposited on the glass, and in an hour and a quarter the process is complete. The liquid is poured off, the glass washed with distilled water, dried, and covered with a varnish to preserve the silver from friction. The advantages are evident. Mercury with its sanitary evils is suppressed; there is a gain in point of cost, as 4 to 5 grms. of silver, costing about 1 franc, suffice for 1 square metre, which, under the old system, would require 700 grms. of tin and the same weight of mercury. A few hours suffice to finish a glass on the new system, whilst the old process required twelve days as a minimum. On the other hand, the glasses thus silvered have a more yellowish tint; portions of the pellicle of silver sometimes become detached, especially if exposed to the direct action of the sun, and despite the protecting varnish the silver is sometimes blackened by sulphuretted hydrogen. M. Lenoir has happily succeeded in overcoming these defects by a process alike simple and free from objections on sanitary grounds. The glass, silvered as above, is washed, and then sprinkled with a dilute solution of the double cyanide of mercury and potassium. The silver displaces a part of the mercury and enters into solution, whilst the rest of the silver forms an amalgam whiter and much more adhesive to glass than pure silver. The transformation is instantaneous. The amount of mercury fixed does not exceed 5 to 6 per cent. The glass thus prepared is free from the yellowish tint of pure silver: it is also less attacked by sulphur vapours and the rays of the sun, in which last respect it is superior to mirrors silvered by the old process.

An examination of French red wines for genuineness of colour has been made by M. Eugen Dietrich. The red of genuine wines appears brownish in thin layers; if diluted with 50 parts of water its colour is very faint; whilst artificially coloured wines appear decidedly blue-red, even at such a degree of dilution. If diluted with 20 parts by weight of water the behaviour of genuine wines as compared with spurious was as follows:—*Acetate of Lead* 1 : 10.—In genuine wines colour disappears; liquid becomes dirty and turbid. On heating, small silver-grey flakes with a slight reddish tint appear. Spurious wines yield large curdy flakes of a deep violet-blue, which on heating becomes more decided. *Sulphate of Copper* 1 : 10.—In genuine wines the colour disappears almost entirely; without turbidity. Spurious wines turn violet-blue, with a slight turbidity. *Baryta Water* 1 : 10.—Genuine wines lose their colour almost entirely; faint turbidity. Spurious wines turn violet-blue, or greenish blue and turbid. Filter-paper steeped in the above tests, and dried, becomes colourless if moistened with genuine wine, whilst coloured sorts give a violet or blue spot.